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APPLICATION NO.	FILING DATE	FIRST NAMED INVENTOR	ATTORNEY DOCKET NO.
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09/501,621 02/09/00 MILLS

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EXAMINER

020736 IM52/0807  
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WASHINGTON DC 20036-3307

LANGEL, W	
ART UNIT	PAPER NUMBER

1754  
DATE MAILED:

08/07/01

Please find below and/or attached an Office communication concerning this application or proceeding.

Commissioner of Patents and Trademarks

# Office Action Summary

Application No.

501621

Applicant(s)

Mills

Examiner

Langel

Group Art Unit

1754

—The MAILING DATE of this communication appears on the cover sheet beneath the correspondence address—

## Period for Reply

A SHORTENED STATUTORY PERIOD FOR REPLY IS SET TO EXPIRE 3 MONTH(S) FROM THE MAILING DATE OF THIS COMMUNICATION.

- Extensions of time may be available under the provisions of 37 CFR 1.136(a). In no event, however, may a reply be timely filed after SIX (6) MONTHS from the mailing date of this communication.
- If the period for reply specified above is less than thirty (30) days, a reply within the statutory minimum of thirty (30) days will be considered timely.
- If NO period for reply is specified above, such period shall, by default, expire SIX (6) MONTHS from the mailing date of this communication.
- Failure to reply within the set or extended period for reply will, by statute, cause the application to become ABANDONED (35 U.S.C. § 133).

## Status

- ☒ Responsive to communication(s) filed on applicant's communications of 7-10-01 and 7-13-01
- ☒ This action is **FINAL**.
- ☐ Since this application is in condition for allowance except for formal matters, **prosecution as to the merits is closed** in accordance with the practice under *Ex parte Quayle*, 1935 C.D. 1 1; 453 O.G. 213.

## Disposition of Claims

- ☒ Claim(s) 1-271 is/are pending in the application.
- Of the above claim(s) \_\_\_\_\_ is/are withdrawn from consideration.
- ☐ Claim(s) \_\_\_\_\_ is/are allowed.
- ☒ Claim(s) 1-271 is/are rejected.
- ☐ Claim(s) \_\_\_\_\_ is/are objected to.
- ☐ Claim(s) \_\_\_\_\_ are subject to restriction or election requirement.

## Application Papers

- ☐ See the attached Notice of Draftsperson's Patent Drawing Review, PTO-948.
- ☐ The proposed drawing correction, filed on \_\_\_\_\_ is ☐ approved ☐ disapproved.
- ☐ The drawing(s) filed on \_\_\_\_\_ is/are objected to by the Examiner.
- ☐ The specification is objected to by the Examiner.
- ☐ The oath or declaration is objected to by the Examiner.

## Priority under 35 U.S.C. § 119 (a)-(d)

- ☐ Acknowledgment is made of a claim for foreign priority under 35 U.S.C. § 119(a)-(d).
  - ☐ All ☐ Some\* ☐ None of the CERTIFIED copies of the priority documents have been received.
  - ☐ received in Application No. (Series Code/Serial Number) \_\_\_\_\_.
  - ☐ received in this national stage application from the International Bureau (PCT Rule 1 7.2(a)).

\*Certified copies not received: \_\_\_\_\_

## Attachment(s)

- ☒ Information Disclosure Statement(s), PTO-1449, Paper No(s). 4, 7 and 10 ☐ Interview Summary, PTO-413
- ☒ Notice of Reference(s) Cited, PTO-892 ☐ Notice of Informal Patent Application, PTO-152
- ☐ Notice of Draftsperson's Patent Drawing Review, PTO-948 ☐ Other \_\_\_\_\_

Office Action Summary

Art Unit '1754

Claims 1-271 are rejected under 35 U.S.C. § 112, second paragraph, as being indefinite for failing to particularly point out and distinctly claim the subject matter which applicant regards as the invention. It is indefinite as to what would constitute an "increased binding energy hydrogen species" (i.e., increased over what?) It is also indefinite as to what would constitute "a desired energy level", "corresponding ordinary hydrogen species", and "ordinary hydrogen species".

35 U.S.C. § 101 reads as follows:

Whoever invents or discovers any new and useful process, machine, manufacture, or composition of matter or any new and useful improvement thereof, may obtain a patent therefore, subject to the conditions and requirements of this title.

Claims 1-271 are rejected under 35 U.S.C. § 101 because the disclosed invention is inoperative and therefore lacks utility. See the reasons given in the last Office action.

Claims 1-271 are rejected under 35 U.S.C. § 112, first paragraph, as containing subject matter which was not described in the specification in such a way as to enable one skilled in the art to which it pertains, or with which it is most nearly connected, to make and/or use the invention. See the reasons given in the last Office action.

Applicant's arguments filed July 10, 2001 have been fully considered but they are not deemed to be persuasive. See the ATTACHMENT TO RESPONSE TO APPLICANT'S ARGUMENTS. Moreover, the

Serial No. 09/501,621

-3-

Art Unit 1754

Rule 132 Declaration filed July 10, 2001 by applicant Mills cites "exhibits" which were previously already submitted along with other exhibits or attachments with the amendment filed March 12, 2001. These exhibits or attachments have been fully considered as evidenced by such ATTACHMENT. See the footnote on page 4 of the ATTACHMENT.

**THIS ACTION IS MADE FINAL.** Applicant is reminded of the extension of time policy as set forth in 37 C.F.R. § 1.136(a).

A SHORTENED STATUTORY PERIOD FOR RESPONSE TO THIS FINAL ACTION IS SET TO EXPIRE THREE MONTHS FROM THE DATE OF THIS ACTION. IN THE EVENT A FIRST RESPONSE IS FILED WITHIN TWO MONTHS OF THE MAILING DATE OF THIS FINAL ACTION AND THE ADVISORY ACTION IS NOT MAILED UNTIL AFTER THE END OF THE THREE-MONTH SHORTENED STATUTORY PERIOD, THEN THE SHORTENED STATUTORY PERIOD WILL EXPIRE ON THE DATE THE ADVISORY ACTION IS MAILED, AND ANY EXTENSION FEE PURSUANT TO 37 C.F.R. § 1.136(a) WILL BE CALCULATED FROM THE MAILING DATE OF THE ADVISORY ACTION. IN NO EVENT WILL THE STATUTORY PERIOD FOR RESPONSE EXPIRE LATER THAN SIX MONTHS FROM THE DATE OF THIS FINAL ACTION.

Any inquiry concerning this communication should be directed to Wayne A. Langel at telephone number (703) 308-0248.

WAL:cdc

August 1, 2001

**WAYNE LANGEL  
PRIMARY EXAMINER  
GROUP 110**

# ATTACHMENT TO RESPONSE TO APPLICANT'S ARGUMENTS

## CONTENTS

<i>Section</i>	<i>Page</i>
List of symbols	3
§ 1. Introduction	4
§ 2. Examiner's position and state-of-the-art recognition of the validity of quantum mechanics	6
§ 3. Response to applicant's position that "classical physics describes reality on <i>all</i> scales"	8
§ 4. Response to applicant's position that his theory is based on "first principles"	11
§ 5. Applicant's use of a classical wave equation to describe quantum phenomena is incorrect	12
§ 6. Applicant's use of a boundary condition, based on a paper by Haus, is unjustified	14
§ 7. Applicant's explanation for the stability of the "hydrino atom" is unpersuasive	16
§ 8. Applicant's use of classical electromagnetic theory in the context of the hydrogen atom is incorrect	20
§ 9. Applicant's derivation of the energy states of the "hydrino atom" has no basis	20
§ 10. Applicant's use of a Dirac delta function to represent a charge density is incorrect	23
§ 11. Applicant's theory predicts irreproducible phenomena, such as "cold fusion," just as equally well as the "hydrino atom"	25
§ 12. Applicant states that the Schrödinger equation does <i>not</i> predict "cold fusion"	26
§ 13. Response to the argument that "those skilled in the art know that the Schrödinger equation does not represent physical reality"	26
§ 14. Response to the argument that the Heisenberg Uncertainty Principle "fails"	28

<i>Section</i>	<i>Page</i>
§ 15. Response to the argument that Feynman's derivation of the Bohr radius is "flawed"	31
§ 16. Applicant's description of the prior art is incorrect	34
§ 17. Response to the argument that the Schrödinger equation "fails" and that Dirac's quantum electrodynamics is "fatally flawed"	37
§ 18. Applicant's linkage of a "crisis" in quantum mechanics with the shattering of the doctrine of the big bang origin of the universe is erroneous	46
§ 19. The Turner and Dhandapani Declarations with respect to NMR experimental data lack probative value	47
§ 20. Calorimetric experiments purporting to demonstrate "apparent excess heat" cannot be accepted <i>uncritically</i>	49
§ 21. Applicant's calorimetric experimental data are not persuasive	50
§ 22. Accurate calorimetric experiment disproves applicant's thesis that apparent excess heat requires an explanation in terms of the postulated "hydrino atom"	53
§ 23. The Phillips Declaration with respect to "hydrino" formation in a calorimeter lacks probative value	54
§ 24. Applicant's spectroscopic and plasma experimental data are not persuasive	56
§ 25. The Mills Declaration with respect to "extreme ultraviolet spectroscopic data" lacks probative value	61
§ 26. Conclusion	66
State-of-the-art books and journals cited by the examiner	67

### List of symbols

- $a$  Bohr radius = Radius of the innermost orbit of the electron in the hydrogen atom =  $\hbar^2/m\epsilon^2 = 5.2917 \times 10^{-9}$  cm. = 0.52917 Å.
- $c$  Velocity of light = 299793 km/sec.
- $e$  Charge of the electron =  $4.8029 \times 10^{-10}$  esu.
- $E$  Energy.
- $\mathcal{H}$  Hamiltonian operator  $\equiv -(\hbar^2/2m)\nabla^2 + V(\mathbf{r})$  (where the symbols are defined below).
- $\hbar$  Planck's constant  $(h)/2\pi = 1.0544_5$  erg sec.
- $i$   $\sqrt{-1}$ .
- $L_n^m$  Associated Laguerre function.
- $m$  Mass of the electron =  $9.108_5 \times 10^{-28}$  gm.
- $n$  &  $l$  Principal & angular momentum quantum numbers, respectively.
- $\mathcal{R}$  Rydberg unit of energy =  $\alpha^2 mc^2/2 = 13.605_0$  eV (electron volt).
- $t$  Time coordinate.
- $V(\mathbf{r})$  Potential energy (bolded symbol indicates a radial space coordinate vector).
- $x, y, z$  Cartesian space coordinates.
- $Z$  Atomic number. Nuclear charge.
- $\alpha$  Sommerfeld's fine structure constant =  $e^2/\hbar c = 1/137.037$ .
- $\gamma$  Square of the wave velocity (Schiff (1968)).
- $\delta(r)$  Dirac delta function.
- $\nabla^2$  Laplacian operator  $\equiv \nabla \cdot \nabla \equiv \partial^2/\partial x^2 + \partial^2/\partial y^2 + \partial^2/\partial z^2$  (bolded symbol denotes the "del" operator and " $\cdot$ " stands for the dot or scalar product).
- $\rho$  A scaled radial space coordinate (Pauling and Wilson's notation (1985)).  
Charge density function of the electron (Applicant's notation).
- $\psi$  Wave function.

## 1. Introduction

Applicant's arguments together with all the attachments filed March 1, 2001 and the Mills declaration filed June 8, 2001 have been fully considered but they are not persuasive.<sup>†</sup> It is noted that applicant's claims contain a limitation referring to the energy states of an electron in a hitherto unheard-of and unknown form of hydrogen atom called a "hydrino atom." Applicant has presented a theory of the "hydrino atom" and has alleged that his submitted experimental results could be interpreted in terms of the energy states of the "hydrino atom." The applicant has also submitted *inter alia* an attachment (number 27) which shows that his theory predicts "cold fusion" as well. Since the *same theory of applicant's* that is used in the instant application to provide support for the alleged existence of the "*hydrino atom*" *predicts equally well irreproducible phenomena such as "cold fusion,"* the scientific basis of applicant's theory/experimental results and the *reproducibility* of the experimental results are questionable.

In the previous office action, the examiner rejected all the claims as not being enabled under 35 USC 112, 1<sup>st</sup> paragraph and as lacking credible utility under 35 USC 101. In addition to grounding the rejections in well-established cases *In re Wands*, 858 F.2d 731, 737, 8 USPQ2d 1400, 1404 (Fed. Cir. 1988) and *Newman v. Quigg*, 681 F.Supp. 16, 5 USPQ2d 1880, 1882 (D.D.C. 1988), the examiner set forth a *prima facie* case denying the existence of the "hydrino atom" and, in an Appendix attached to the action, gave an account of the

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<sup>†</sup> Hereafter, all references to an amendment, attachments and declarations pertain to the amendment, attachments and declarations filed March 1, 2001 and the Mills declaration filed June 8, 2001 in Serial No. 09/009,294. The principal arguments in the amendments filed in various related applications (09/008,947; 111,160; 009,455; 110,678; 009,837; 110,694; 110,717; 111,003; 225,687; 362,693) connected with the "hydrino atom" are essentially the same.



conventionally accepted theory of the hydrogen atom to demonstrate that there is *no basis for the "hydrino atom."*

While it is agreed that an inventor need not necessarily understand the theory behind his invention, attention is drawn to the fact that by reciting the phrases that include "hydrino atom" or equivalent terminology, such as, "hydrino hydride," "increased binding energy hydrogen species," etc. referring to something other than a "normal" hydrogen atom, in his claims, *the applicant has ipso facto introduced his theory of the "hydrino atom" into the claims.*

Moreover, a similar interpretation of an invention occurred in *Newman v. Quigg, op. cit.*, where an applicant's claims to a machine which operated according to a theory which violated the second law of thermodynamics were held to be unpatentable. Hence it is clear why the examiner is obliged to review applicant's theory in addition to evaluating the experimental evidence alleged to support patentability of the present claims.

The applicant's theory is the *unique* source from which the existence of the "hydrino atom" is demonstrated. It is, therefore, natural and logical to interpret applicant's invention in terms of the basic underlying premise offered by applicant's theory.

The specification (see, e.g., p. 3 in Serial No. 09/009,294 under "Background Of The Invention") mentions applicant's book<sup>\*</sup> as providing a "theoretical introduction" to applicant's invention. The examiner has, therefore, consulted that book to better understand applicant's invention.

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<sup>\*</sup> Mills, R. L. (2000) *The Grand Unified Theory of Classical and Quantum Mechanics*, Blacklight Power Inc., New Jersey. This book was submitted as attachment 16 with the amendment filed, e.g., March 1, 2001 in Serial No. 09/009,294. Hereafter, all references to the applicant's book are to this one.

## 2. Examiner's position and state-of-the-art recognition of the validity of quantum mechanics

The examiner's principal position is that applicant has failed to scientifically substantiate the very existence of the "hydrino atom" let alone its many alleged manifestations in the experimental data provided by the applicant. It is noted that scientific research on the hydrogen atom extends over nearly a century. The relatively "simple" structure of the hydrogen atom, which consists of a single electron bound to a single nuclear proton *via* a Coulomb potential, has made it particularly amenable to investigation by the sophisticated techniques of modern science. The very detailed knowledge thus obtained about the properties of the hydrogen atom epitomizes the success of modern science. Experimental measurements of the energy states of the electron in a hydrogen atom are confirmed exactly by calculations based on a well-accepted theory known as quantum mechanics (or, formerly, wave mechanics).

Since its inception over a century ago, quantum mechanics has been progressively refined to the point that its credentials with respect to making highly accurate predictions are very well established. Despite certain *philosophical* conundrums, one of skill in the art would agree that, *in actual practice*, quantum mechanics makes highly successful predictions that clearly *do* stand up to experimental verification. As evidence to support this position, note the following quotations from state-of-the-art references:

- "One of the *simplest*, and *most completely treated*, fields of application of quantum mechanics is the theory of *atoms* with *one* or two *electrons*. For *hydrogen* and the analogous ions  $\text{He}^+$ ,  $\text{Li}^{++}$ , etc., the calculations can be performed *exactly*, both in *Schrödinger's nonrelativistic wave mechanics* and in *Dirac's relativistic theory of the electron*. More

specifically, the calculations are *exact for a single electron in a fixed Coulomb potential*.

*Hydrogen-like atoms* thus furnish an *excellent way* of testing the *validity of quantum*

*mechanics.*" Bethe<sup>‡</sup> and Salpeter (1977) on p. 2.

- "The energy formula (2.11) [i.e.  $E = - (1/2) Z^2 / n^2$ ,  $n$  a *positive integer*, representing the energy of the hydrogen atom and hydrogen-like ions having a single electron] has been *verified to an extraordinary degree by spectroscopic measurements.*" Bethe and Salpeter on p. 9.
- "After applying corrections for fine structure, etc., *the agreement for a large number of spectral lines is excellent*, to better than one part in a million for the  $H_\beta$  line, for instance. ... *Many spectral lines* have also been measured for hydrogen-like ions of higher nuclear charge  $Z$ , up to seven-times ionized oxygen ( $Z = 8$ ). *The agreement is again excellent.*" Bethe and Salpeter on p. 12.
- "At the present stage of human knowledge, *quantum mechanics can be regarded as the fundamental theory of atomic phenomena.*" Schiff (1968) very first sentence on p. 1.
- "Quantum theory is *the most precisely tested and most successful theory in the history of science.*" Kleppner & Jackiw (2000) on p. 893.
- "In short, the *experimental verdict is in: the weirdness of the quantum world is real, whether we like it or not.*" Tegmark and Wheeler (2001) on p. 72.

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<sup>‡</sup> H. A. BETHE, NOBEL PRIZE FOR PHYSICS, 1967, FOR HIS CONTRIBUTIONS TO THE THEORY OF NUCLEAR REACTIONS ESPECIALLY HIS DISCOVERIES CONCERNING THE ENERGY PRODUCTION OF STARS.

### 3. Response to applicant's position that "classical physics describes reality on *all* scales"

The applicant's position, as stated in his amendment (p. 53), is that "classical physical laws describe reality on *all* scales." Emphasis added. The conventional meaning of "classical" is that it refers to nonquantum or nonrelativistic physics such as it stood in the 19<sup>th</sup> century or about a hundred years ago.

It is the examiner's position that one of skill in the art would find applicant's viewpoint that a *classical* theory can be applied to study *quantum* phenomena at the microscopic level, such as the behavior of electrons in atoms, to be *fundamentally erroneous*.

As evidence, the examiner draws attention to Tolman (1979), pp. 180-188, who, in a concise and masterly exposition of the elements of quantum mechanics, describes how and why *the necessity for modifying classical ideas* arose in the beginning of the 20<sup>th</sup> century. Quoting Tolman:

"The other of the two difficulties for nineteenth-century physics was that of explaining the failure of electromagnetic energy to distribute itself uniformly over all the possible modes of vibration in an enclosure containing radiation which has come to thermal equilibrium. *A reasonably satisfactory solution of this problem, and of others which proved to be associated with it, has only been made possible by a criticism of the very nature of physical observation, with a resulting appreciation of the uncontrolled character of the effects that measurement itself must produce on systems – particularly microscopic ones – when under observation. Our present system of quantum mechanics must be regarded as the ultimate outcome of such criticism.*" See pp. 180-181.

And, further:

“This new idea, that *atoms are characterized by sets of discrete energy levels* so that radiation can be absorbed and emitted in definite quanta, is the feature of the new developments which has led to the name *quantum mechanics*. Its introduction marks a considerable step away from classical ideas since there was *nothing in the classical picture* of an electric oscillator or of a planetary atom which would lead us to *expect that unique properties should be assigned to any particular energy levels chosen out of all the possible ones.*”

Emphasis added. See p. 182.

The examiner has provided the motivation, based on the statements of those of skill in the art or experts in atomic theory, to back up his contention that a *classical* approach to studying atoms cannot supplant quantum mechanics if a truly accurate understanding of the properties of *atoms* is to be attained.

With regard to quantum mechanics, Feynman et al. (1965) on p. 1-1 accurately describe the impact of quantum mechanical concepts on a person encountering it *initially* as follows:

“Because atomic behavior is so *unlike ordinary experience*, it is very difficult to get used to, and it appears peculiar and mysterious to everyone.”

In light of this observation by Feynman<sup>‡</sup>, a renowned expositor and pioneering scientist, it becomes easier to understand how difficult it is for those who are more comfortable with intuitive classical physics to bridge the gap leading to the relatively less intuitive, but more challenging, subtleties of quantum mechanics and to eventually see the latter for the powerful tool that it really is. Once that stage is reached, as in the case of a person of skill in the art, the truth behind Feynman et al.’s statement below (p. 1-1) becomes apparent:

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<sup>‡</sup> R. P. FEYNMAN, NOBEL PRIZE FOR PHYSICS, 1965, FOR HIS FUNDAMENTAL WORK IN QUANTUM ELECTRODYNAMICS, WITH DEEP-PLOUGHING CONSEQUENCES FOR THE PHYSICS OF ELEMENTARY PARTICLES.

“[There is] a phenomenon which is impossible, *absolutely* impossible, to explain in *any* classical way, and which has in it the heart of quantum mechanics.”

Where the phenomenon referred to above is none other than (p. 1-1):

“[T]he *quantum behavior of electrons*.”

In view of the above discussion, it would have come as no surprise to one of ordinary skill in the art that the improper use of a theory, such as applicant's classical theory, to explain phenomena in a quantum domain, such as the behavior of electrons in atoms, can all too easily lead to erroneous conclusions such as the existence of a “hydrino atom.”

It is no easy task to determine a basis for applicant's position in light of the fact that his theory lacks a coherent framework where equations are linked by a logical chain involving proper scientific and mathematical reasoning. As a case in point, see, e.g., pp. 20 and 83 in his book that demonstrate an absence of logical flow in the mathematics and physics of applicant's theory and exhibit mere conclusionary statements for the end result of applicant's theory which is fractional integer values for the principal quantum number,  $n$ , that are not consistent with quantum mechanics.

Applicant holds that such hallmarks of quantum mechanics as the Heisenberg Uncertainty Principle, wave-particle duality, etc. and such philosophical puzzles posed by quantum mechanics as quantum superposition show that conventional quantum mechanics is flawed. In response, it is noted that, in fact, it is a generally recognized characteristic of quantum mechanics that *it continues to be very successful in practice despite some ongoing philosophical issues*. There is no objective evidence that quantum mechanics is either in any danger of being abandoned or considered to be contravened by those recognized for their skill in the art as is clear from Tegmark and Wheeler (2001). Indeed, a measure of the enormously successful

impact of quantum mechanics on industry is apparent in the following quotation from Tegmark and Wheeler at p. 69:

“[T]he astonishing range of scientific and practical applications that quantum mechanics undergirds: today an estimated *30 percent of the U.S. gross national product is based on inventions made possible by quantum mechanics*, from semiconductors in computer chips to lasers in compact-disc players, magnetic resonance imaging in hospitals, and much more.”

Furthermore, as the quotation from Tegmark and Wheeler so tellingly puts it: “*the experimental verdict is in: the weirdness of the quantum world is real, whether we like it or not*” - and this is as of February 2001.

In light of these viewpoints expressed by those of skill in the art, it would appear, on the balance, that the applicant’s opinion that “Quantum theory cited [sic] makes nonsensical, nonphysical predictions ... ” (at p. 16 of the amendment) clearly lacks probative value.

#### **4. Response to applicant’s position that his theory is based on “first principles”**

Applicant has stressed that his theory is based on “first principles.” The basis for this seems to lie in applicant’s use of a classical wave equation supplemented by a certain boundary condition. In contrast, according to the applicant’s own notion, the Schrödinger<sup>‡</sup> equation of quantum mechanics is a “theory which was curve-fitted” (see p. 16 of the amendment). Such an argument is illogical. Both the classical wave equation and the Schrödinger wave equation are descriptive of wave phenomena, with the crucial differences

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<sup>‡</sup> E. SCHRÖDINGER, NOBEL PRIZE FOR PHYSICS, 1933, FOR THE DISCOVERY OF NEW PRODUCTIVE FORMS OF ATOMIC THEORY.

being that the quantum wave function in Schrödinger's equation is interpreted in probabilistic terms and that Schrödinger's equation is applicable in the quantum domain whereas the classical wave equation is not. Following applicant's own logic, it is apparent that a theory based on the Schrödinger equation is entitled to be just as much of a "first principles" theory as is applicant's theory. Therefore, any argument that the applicant's theory is more fundamental or well-founded than, for instance, quantum mechanics based on Schrödinger's equation is without merit.

**5. Applicant's use of a classical wave equation to describe quantum phenomena is incorrect**

Applicant's use of a classical wave equation to describe quantum phenomena is incorrect. Attention is drawn to a state-of-the-art standard graduate level textbook in quantum mechanics by Schiff (1968) which, on page 21, demonstrates the necessity of formulating a wave equation *that is more fundamental and general than the classical wave equation*. See, especially, lines 11-12 and lines 22-23 on p. 21 of Schiff. In particular, *the use of a classical wave equation is to be avoided* for the reason that in a classical wave equation depicted, e.g., by:

$$\partial^2 \psi / \partial t^2 = \gamma \partial^2 \psi / \partial x^2,$$

the second partial derivative with respect to time, i.e.  $\partial^2 \psi / \partial t^2$ , introduces parameters of motion (momentum, energy, frequency, etc.) into  $\gamma$  thus preventing one from being able to superpose solutions that belong to different values of the parameters in order to obtain a general solution. While the above argument is shown in the context of a 1-dimensional classical wave equation, it is equally applicable to the applicant's 3-dimensional classical wave



equation (see equation (1.2) on p. 2 of his book) given the correspondence between the above classical wave equation and applicant's classical wave equation when  $\psi \rightarrow \varrho$  and  $\gamma = v^2$ .

In contrast to the classical wave equation, it turns out that a more fundamental and general equation aptly suited to describe quantum behavior, such as Schrödinger's equation,

$$i\hbar \partial\psi/\partial t = \mathcal{H}\psi \equiv -(\hbar^2/2m) \nabla^2\psi + V(\mathbf{r}) \psi,$$

contains a *first* partial derivative with respect to time,  $\partial\psi/\partial t$ , where such a problem does not arise.

(See p. 3 for the meaning of the above symbols.) By resurrecting a mathematically discredited approach the applicant has introduced a clear error into his theory thereby raising serious questions regarding the credibility of any conclusions derived from his classical wave equation-based theory, such as, the existence of a "hydrino atom."

Incidentally, it is noted that applicant's classical wave equation (cf. equation (1.2) on p. 2 of his book) is *improper* in that the second derivative with respect to time is shown as " $\delta^2/\delta t^2$ " which denotes a second-order finite difference operator. To the examiner's knowledge, there is *no* classical wave equation with such an operator. The standard classical wave equation contains the (infinitesimal) partial differential operator " $\partial^2/\partial t^2$ " which has quite a different connotation, viz.:

$$\partial^2 f(x,t)/\partial t^2 = \lim_{\delta t \rightarrow 0} \left\{ (\delta / \delta t) (\delta f(x,t) / \delta t) \right\} \Big|_{x \text{ const}},$$

where  $f(x,t)$  is a proper test function. Applicant's use of an improper equation is an additional reason as to why the *results* from his *improper* theory cannot be relied upon.

Based on the above discussion, it is reasonable to hold that applicant's improper use of a classical wave equation to describe a quantum mechanical system, such as a hydrogen atom, introduces a fundamental error in his theory which would have caused a person of ordinary skill to question the credibility of any results derived therefrom, such as the "hydrino atom"

characterized by energy levels with fractional integer values for the principal quantum number,  $n$ .

**6. Applicant's use of a boundary condition, based on a paper by Haus, is unjustified**

Applicant's use of a boundary condition, based on a paper by Haus<sup>★</sup> entitled "On the radiation from point charges," is unjustified. In order to place applicant's argument in proper perspective, it is noted that from a purely classical point of view an accelerated charge radiates energy. See, e.g., Jackson (1975) at p. 654. If one were to extend this argument to an electron in a hydrogen atom whose nucleus consists of a single proton, then, *classically speaking*, one would expect the electron to continuously radiate away energy as it spirals towards and eventually collapses into the positively charged nucleus. So, classically speaking again, the hydrogen atom cannot exist. Reality, however, dictates otherwise - hydrogen atoms clearly exist. Their spectra have been measured. What, then, is the explanation for the stability of the hydrogen atom? *Clearly, using classical concepts to describe a hydrogen atom leads to unrealistic conclusions such as the collapse of the atom.*

Not surprisingly, the same problem also occurs in the applicant's theory of the hydrogen atom given its origins in a classical wave equation. It is contended that any argument by the applicant that the failure of the classical wave equation is somehow remedied in his theory by using a boundary condition based on Haus' paper is devoid of merit in light of the following facts:

(i) Haus's paper deals with an electron being a *point* charge whereas, by applicant's own admission on p. 17 of the amendment, in applicant's theory the electron is *not a point charge*.

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<sup>★</sup> Haus, H. (1986) *American Journal of Physics*, vol. 54, no. 12, pp. 1126-1129. See attachment 17.

Therefore, Haus's boundary condition for a point charge *cannot* be applied to applicant's "electron" which is *not* a point charge without producing a scientifically unacceptable result. It is observed that on p. 54 of the amendment, the applicant stated that: "The Haus derivation applies to a moving charge-density function as well because charge obeys superposition."

This raises two issues. Firstly, since the applicant has *not* shown that a *superposition of point charges* is the *same thing* as his "extended" or "two-dimensional" electron, his response falls short of meeting the thrust of the examiner's contention. Secondly, since the applicant *fails* to take into account the self-energy of his electron (or charge-density distribution), or, the interactions that occur between the different (or superposed) components representing the internal structure of his electron, especially, in light of his admission that his electron is not a point charge, it is deemed that applicant has not shown that there is proper scientific basis to apply Haus's boundary condition for a point charge to applicant's electron which is not a point charge.

(ii) Haus's paper is relevant to a *free electron laser* whereas applicant's theory is concerned with an *atom where the electron is not a free electron*. That is, in Haus's paper there is no proton exerting a Coulomb force on an electron as in the case of the hydrogen atom. These issues were previously raised by the examiner on p. 4 of the Appendix attached to the previous office action but have *yet* to receive a satisfactory reply from the applicant. Since the two areas of application (free electron laser and the hydrogen atom) are *prima facie* different and constitute, in essence, *non-analogous art*, once again it is evident that Haus's boundary condition which was used in the context of a free electron laser cannot be bodily

incorporated into a theory of the hydrogen atom without leading to scientifically unacceptable results.

(iii) It is noted that Haus's intent was to provide an *alternative* explanation, based on Fourier transforms, as to why a charge moving at *constant* velocity does not radiate energy. Furthermore, Haus's paper is *confined* to dealing with the equations of *macroscopic* electrodynamics (very first sentence in his paper). Therefore, a *proper* application of Haus's boundary condition to a quantum mechanical system such as the hydrogen atom *cannot* be made unless there is *evidence* that the electron in the hydrogen atom moves at *constant* velocity and a proper analysis is conducted to determine whether, in fact, the equations of *macroscopic* electrodynamics would be applicable to a *microscopic* system such as the hydrogen atom. It is only proper scientific procedure to determine the realm of validity of Haus's boundary condition before hinging one's conclusions on the results obtained therefrom. There is nothing in the record of the existence of such evidence and of such analysis as having been performed.

It is concluded that applicant has applied Haus's derivation in a purely mathematical manner *out of context* to an electron in a quantum mechanical system, such as the hydrogen atom, without proper mathematical and scientific justification.

#### **7. Applicant's explanation for the stability of the "hydrino atom" is unpersuasive**

The examiner had previously raised the issue of the implosion or lack of stability of the "hydrino atom" on p. 4 of the Appendix to the previous office action. In his response, on pp. 57-58 of the amendment, the applicant refers to "conservation of energy" as shown in Chapter 5 of his book as accounting for the alleged stability of the "hydrino atom." The

examiner has consulted Chapter 5, pp. 138-161, in the book but finds the issue to be as yet *unresolved*. Thus pp. 138-159 deal essentially with a catalytic system based on tapping into alleged energy states of the "hydrino atom" and do not come to grips with the issue of the instability of the "hydrino atom" in the first place. Page 160 indicates that "[h]ydrogen atoms can undergo transitions to energy states below the ground state until the total potential energy of the *proton* is converted to relativistically corrected kinetic energy and total energy." Emphasis added. However, it is not understood as to what the potential energy of the *proton* has to do with the energy of an *electron*, especially, when the issues of interest are the energy states of the electron. Moreover, since the total energy *is* the sum of the potential and kinetic energies, no meaning can be ascribed to applicant's statement that the potential energy is converted to *inter alia* total energy. It appears that applicant has given a *confusing* and *conclusionary* statement that alleges that something *can* happen as opposed to just what does happen. It is evident that applicant's response regarding the lack of stability of the "hydrino atom" is clearly unpersuasive.

It proves instructive, at this point, to turn one's attention to Tegmark and Wheeler (2001) who, on page 70, state that the correct explanation for the stability of the hydrogen atom, due to Bohr<sup>‡</sup>, is that an electron in a hydrogen atom exists in any one of a number of stable quantum energy states known as stationary states. *These stationary states are precisely the energy states that are logically obtained by solving Schrödinger's equation subject to proper boundary conditions.* As evidence the examiner refers to pp. 19-1 to 19-18 in Feynman et al. (1965) which was cited and attached to the Appendix in the previous office action. The *lowest* energy

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<sup>‡</sup> N. BOHR, NOBEL PRIZE FOR PHYSICS, 1922, FOR HIS SERVICES IN THE INVESTIGATION OF THE STRUCTURE OF ATOMS AND OF THE RADIATION EMANATING FROM THEM.

state occupied by the electron is known as the *ground state* and is characterized by the quantum numbers  $n = 1$  and  $l = 0$ . It stands to reason that there cannot be any state lower than, i.e. below, the ground state. Therefore, applicant's statement that "[h]ydrogen atoms can undergo transitions to energy states below the ground state" on p. 160 of his book is incorrect. While occupying a stationary state the electron does not radiate energy. That is why the hydrogen atom does not collapse - just as nature has it. There is no need at all to invoke extraneous equations and borrow special boundary conditions *since the Schrödinger equation is the proper equation, within a non-relativistic spin-free framework, describing the behavior of the electron and suffices to answer scientifically meaningful questions about the quantum energy levels of the electron.*

Previously, the examiner had previously set forth Feynman et al.'s elucidation of the stability of the hydrogen atom based on an application of the Heisenberg Uncertainty Principle. See Endnote 5 in the Appendix attached to the previous office action.

Parenthetically, it is noted that the Schrödinger equation, in the cited description to Feynman et al. (1965) at pp. 19-1 to 19-18, does not account for the corrections that would be expected if such factors as special relativity and the spin property of the electron were taken into account. However, the emendations introduced by consideration of the above factors account for but a *minute*<sup>★</sup> portion of the total (binding) energy of the electron in the ground state thereby confirming the *fundamentally correct* picture of matter at the atomic level described by Schrödinger's equation. The Dirac equation accounts for both of the above mentioned factors. The equations of quantum electrodynamics go beyond even the Dirac

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★ About one part in one hundred thousand, i.e. 0.001%, of the total (binding) energy as described further in § 17 below.

equation in that they account for the self-energy (or self-interaction) of the electron.

Moreover, there is a *logical* development in the passage from the Schrödinger equation to the Dirac equation and, thence, to the equations of quantum electrodynamics. These issues are discussed in § 17 below.

It is extremely important to recognize that *even without* the introduction of the *minute corrections* stemming from considering special relativity and the spin of the electron, the use of the Schrödinger wave equation, as opposed to the classical wave equation, represents a *major departure in the right direction away from purely classical ideas* as substantiated by over 100 years of scientific research into the behavior of *electrons in atoms*.

The elegance and power of quantum mechanics is especially evident in its application to the simplest of atoms - the hydrogen atom. Applicant's attention is again drawn to pp. 19-1 to 19-18 in Feynman et al. (attached to the previous office action) which describes the *true* properties of the hydrogen atom.

In conclusion, it is apparent that applicant's invocation of Maxwell's equations *via* a boundary condition based on Haus's paper achieves nothing constructive since it is both erroneous and unnecessary for an electron in a stationary state and since, as stated previously, applicant's classical wave equation is simply inapplicable to a quantum mechanical system such as the hydrogen atom. It is observed that only when an electron transits from a higher energy stationary state to a lower energy stationary state does it radiate energy having a frequency given by the difference in energy between the two states divided by Planck's constant.

**8. Applicant's use of classical electromagnetic theory in the context of the hydrogen atom is incorrect**

Applicant's use of classical electromagnetic theory in the context of the hydrogen atom is not in accordance with the proper criteria by which one of skill in the art would have decided whether classical or quantum theory is applicable. As evidence attention is directed to Jackson (1975) at p. 4 which states that:

"The *quantum* nature of the electromagnetic radiation must, on the other hand, be taken into account in spontaneous emission of radiation by atoms, *or by any other system where there are no photons present initially and only a small number of photons present finally.*"

Since the hydrogen atom contains a proton, an electron and *no* photons initially, it is evident that quantum, *not classical*, electromagnetic theory must be employed, contrary to applicant's approach.

**9. Applicant's derivation of the energy states of the "hydrino atom" has no basis**

Applicant's derivation of energy states of the "hydrino atom" which are characterized by *fractional* quantum numbers ( $1/n$ , where  $n = 2, 3, \dots$ ) has *no proper scientific or mathematical basis*.

The examiner had raised this issue in the previous office action. The present amendment does *not* respond to this other than to set forth a *conclusionary* statement that makes *no mention of the details* of the mathematical steps and the scientific basis for obtaining such improper fractional quantum numbers. See p. 60 of the amendment. Given the applicant's silence on this crucial issue, the examiner referred to applicant's book, in attachment 16, to again find only conclusionary statements. As evidence, pp. 20, 83 and 147 in the book are cited.



On the other hand, applicant has criticized the solutions of the Schrödinger equation which permit *only positive integer values for the principal quantum number,  $n$* . Applicant's position is that "[t]hose skilled in the art, however, readily understand that the Schrödinger equation provides an infinite number of solutions, most of which are not even integers. Positive integer solutions are only obtained by arbitrarily defining a parameter in the Schrödinger equation." See pp. 15-16 in the amendment.

Applicant's argument is deemed to be lacking in scientific merit. As evidence applicant is directed to "Endnote 1" in the Appendix attached to the previous office action. It is observed that *not every mathematically possible solution to the Schrödinger equation leads to a physically meaningful description*. In solving Schrödinger's differential equation for the wave function of an electron in the hydrogen atom a divergent series (i.e. a scientifically meaningless) solution for the wave function is obtained for large values of the radial coordinate (where common sense suggests that the electron in the *ground* state of the hydrogen atom is less likely to be found) *unless the principal quantum number  $n$  is constrained to be a positive natural integer ( $n \geq 1$ )*, not a fraction of an integer as alleged by the applicant. This holds true whether the differential equation is solved directly by a power series solution as shown in Pauling and Wilson (1985) on pp. 121-124 and 140 and as previously set forth by the examiner in Endnote 1 of the previous office action, or, whether the differential equation is transformed into a confluent hypergeometric equation such as Whittaker's equation whose solution can be expressed in terms of the associated Laguerre functions,  $e^{-\rho/2} \rho^l L_{n+l}^{2l+1}(\rho)$ , as shown in Jeffreys and Jeffreys (1950) at p. 618 between equations (6) and (7).

Further support is found in Margenau and Murphy (1943), at pages 77-78, from which it is evident that when  $n$  is not a positive integer but any constant  $p$  then a physically meaningful

solution cannot be expressed as  $e^{-x/2} x^{(k-1)/2} (d^k/dx^k) L_p(x)$ . The reason for this is very simple, *viz.*, when  $n$  is not a positive integer then, according to Margenau and Murphy themselves at p. 78 *op. cit.*,

*“this solution would no longer be a polynomial in  $x$  multiplied by  $e^{-x/2}$  but an infinite sequence.”* Emphasis added.

Such an infinite sequence, which has *no physical significance*, stems from  $n$  being anything other than a positive integer, such as the fractional integer espoused by applicant. It does *not* provide for *a physically meaningful solution to the Schrödinger equation*. Consequently, it is clear that the use of fractional quantum numbers can only produce unscientific results.

To summarize, in the cited endnote of the previous office action, the examiner stated that it is the imposition of a boundary condition requiring the wave function to have a proper asymptotic behavior at infinity, i.e. the wave function must be finite there, that causes truncation of the infinite power series expansion for the wave function to a finite polynomial solution and at the same time the truncation leads to only *positive integer* values for the principal quantum number  $n$ . There is *nothing arbitrary about such a well-defined mathematical procedure that produces a correct solution for the hydrogen atom*. For a complete exposition of the *proper* mathematical procedure for solving the Schrödinger equation see Pauling<sup>‡</sup> and Wilson (1985) at pp. 121 to 124, 140 and Feynman et al. (1965) at pp. 19-1 to 19-18 attached to the

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<sup>‡</sup> L. C. PAULING, NOBEL PRIZE FOR CHEMISTRY, 1954, FOR HIS RESEARCH INTO THE NATURE OF THE CHEMICAL BOND AND ITS APPLICATION TO THE ELUCIDATION OF THE STRUCTURE OF COMPLEX SUBSTANCES.

Appendix in the previous office action. In view of the above discussion, it is apparent that the fractional integer energy levels of the "hydrino atom" have no proper basis.

It follows from the above discussion that since there is no support for the existence of fractional integer levels such as, e.g.  $n = 1/2$ , in the hydrogen atom, the applicant's statement, at p. 61 of his amendment, that "a transition between two nonradiative states is *possible via* a nonradiative energy transfer, say  $n = 1$  to  $n = 1/2$ " has no scientific merit.

#### 10. Applicant's use of a Dirac delta function to represent a charge density is

**incorrect**

Applicant's use of a Dirac delta function,  $\delta(r-r_n)$ , to represent the radial function, and thus also the charge density which, according to applicant, is directly related to the radial function, of an electron (see p. 3 of his book as well as equation (39) as cited on p. 55 of the amendment) is scientifically improper. The reason for this is that the Dirac delta function has *no* physical significance in and of itself. It is a limit of integrals of a sequence and is *meaningful only under an integral sign*. That is:

$$\int_{-\infty}^{\infty} dr \delta(r) f(r) = \lim_{n \rightarrow \infty} \int_{-\infty}^{\infty} dr \delta_n(r) f(r),$$

where  $\delta(r)$ ,  $\delta_n(r)$  and  $f(r)$  are a Dirac delta function, a delta sequence function and a proper test function, respectively. To make matters more difficult, it turns out that the integral on the left-hand side of the above equation is *not* a usual integral in the Riemannian sense. See Arfken (1970) at p. 415, equation (8.85b). An example of a delta sequence function is

$$\delta_n(r) = (\sin nr) / \pi r.$$

See Arfken at p. 414, equation (8.83d). Since the Dirac delta function is a "singular" function it cannot, strictly speaking, be "plotted." However, the delta sequence function above can

certainly be graphed as shown in Fig. 8.3d on attached p. 414 from Arfken. The mathematical underpinning for the Dirac delta function is provided by the Theory of Distributions (due to L. Schwartz, *Théorie des distributions*, Paris, 1951) where the delta function arises as a generalized function or functional. See Arfken at p. 415. It is apparent, based on the above discussion, that mathematical manipulations involving the Dirac delta function are so delicate that it is very easy to be led astray.

In contrast to a point charge which may be “represented” by a Dirac delta function, the charge density is a smeared-out distribution. This issue was raised on p. 3 of the Appendix attached to the previous office action. Applicant’s response on pp. 52-53 of the amendment does not address this point. Applicant states that his electron is an “extended particle - not a point source” (p. 53). This only emphasizes the correctness of the examiner’s position. By definition, the Dirac delta function is an “infinitely high, infinitely thin spike” representing the “charge density for a *point* charge.” See Arfken at p. 413. Therefore, it is mathematically improper to use a Dirac delta function appropriate for a point charge to represent an “extended particle” which, as applicant stated, is not a point source. Since the charge density, and thus the radial function, is a solution to applicant’s classical wave equation for an “extended particle,” the use of a Dirac delta function appropriate for a point charge vitiates applicant’s theory.

**11. Applicant's theory predicts irreproducible phenomena, such as "cold fusion," just as equally well as the "hydrino atom"**

Applicant has submitted a *paper co-authored by the applicant* Mills and Kneizys\* entitled "Excess heat production by the electrolysis of an aqueous potassium carbonate electrolyte and the implications for cold fusion" which is included as attachment 27 in the amendment. In this paper, equations (1) to (3) are *identical in all respects* to the equations that form the basis for predicting the fractional quantum energy states of an electron in the "hydrino atom." Observing that the phrases "shrunk atoms" (abstract of the paper) or "shrunk hydrogen atoms" (p. 81 of the paper) are synonymous with the "hydrino atom," particular note should be made of the following quotation in the abstract of the paper:

"According to a *novel atomic model*, the predominant source of heat of the phenomenon called *cold fusion is the electrocatalytically induced reaction whereby hydrogen atoms undergo transitions to quantized energy levels of lower energy than the conventional ground state. These lower energy states correspond to fractional quantum numbers.*" Emphases added.

Since it is not understood as to how applicant can logically *disavow* cold fusion predicted by his theory (see, e.g., the "Foreword" on p. xi of his book in attachment 16) and *at the same time endorse* a "hydrino atom" predicted from the *very same theory*, it is apparent that troubling questions remain regarding the validity of the applicant's theory. Given the direct connection between cold fusion and the "hydrino atom" in light of the fact that both are *tightly linked via* the applicant's theory of "hydrino atom" having fractional quantum number energy states, should applicant argue otherwise, it of interest to note that in *In re Swartz*, 56 USPQ2d 1703,

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\* Mills, R. L. and Kneizys, S. P. (1991) *Fusion Technology*, vol. 20, pp. 65-81. See attachment 27.

1704 (Fed. Cir. 2000) the court found Swartz's argument that his claims were directed to a process other than cold fusion to be unpersuasive. Swartz's claims to an invention in the area of cold fusion were affirmed to be non-enabling and lacking in utility.

## **12. Applicant states that the Schrödinger equation does *not* predict "cold fusion"**

It is of interest that on p. 65 in applicant Mills and Kneizys's paper (attachment 27 cited in the preceding section), the *Schrödinger equation* was criticized on the grounds that it "*does not explain the phenomenon referred to as 'cold' nuclear fusion.*" In light of that, it would appear to be logical to conclude that the correctness of the Schrödinger equation is thereby confirmed precisely because it does *not* predict an irreproducible phenomenon, such as cold fusion, from which applicant has distanced himself, whereas, in stark contrast, it is the applicant who established (over 16 pages in his paper) that his theory of the "hydrino atom" *does* make such a prediction. The latter forms no solid foundation upon which a proper scientific theory should rest.

## **13. Response to the argument that "those skilled in the art know that the**

### **Schrödinger equation does not represent physical reality"**

On pp. 15 and 42 of the amendment, applicant cites Fuchs and Peres,<sup>★</sup> in a paper entitled "Quantum theory needs no 'interpretation'," as saying "Quantum theory does *not* describe physical reality." Applicant's implication is that quantum mechanics and, in particular, the Schrödinger equation are somehow undermined. To show the error in this

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<sup>★</sup> Fuchs, C. A. and Peres, A. (2000) *Physics Today*, pp. 70-71. See attachment 57.

argument, the examiner sets forth below his own analysis of Fuchs and Peres's paper. What the applicant omitted to include is that in the sentence immediately preceding the above-quoted portion, Fuchs and Peres state that:

“[T]rying to fulfill a *classical worldview* by *encumbering* quantum mechanics ... without *any improvement in its predictive power, only gives the illusion of better understanding.*” Emphasis added.

And, again, at p. 71 *op. cit.* they state:

“However, to make quantum mechanics a useful guide to the phenomena around us, we need nothing more than *the fully consistent theory we already have*. Quantum theory needs no *‘interpretation.’*”

In other words, Fuchs and Peres are *not* critical at all of quantum mechanics provided it is understood as being *useful in an operational or algorithmic sense*, rather than philosophically. Instead, they are alerting the reader to the fact that a classical viewpoint is more of a *hindrance* when intermingled with quantum mechanics in that it leads one to the illusory feeling of having an improved theory when, in fact, such is not the case. Seen in its proper context, Fuchs and Peres' cautionary statement is a warning against making a *naïve philosophical comparison* between physical reality and quantum mechanics.

It is noted that the probabilistic aspects of quantum mechanics created considerable interest among the leading scientists of the day. In order to provide a proper counterbalance to applicant's allegations regarding the foundations of quantum mechanics, attention is directed to pp. 64-85 of Fine (1986) which provides an appraisal of some of these characteristics of quantum mechanics based on a deep and insightful analysis of correspondence between Einstein and Schrödinger on these issues. It is observed that the

pragmatic success of quantum mechanics is never in doubt despite ongoing philosophical exchanges.

#### 14. Response to the argument that the Heisenberg Uncertainty Principle “fails”

Applicant argues on p. 28 of the amendment that the Heisenberg<sup>‡</sup> Uncertainty Principle, a cornerstone of quantum mechanics, has been “demonstrated experimentally to fail” citing Dürr et al. (see attachment 58). The examiner disagrees.

By way of introduction, it is noted that according to one way of stating the Heisenberg Uncertainty Principle:

“[I]t is *impossible* to specify *precisely and simultaneously* the values of both members of particular pairs of physical variables (known as ‘canonically conjugate variables’) that describe the behavior of an atomic system.” Emphasis added.

See p. 7 of Schiff (1968). For canonically conjugate variables such as the position ( $x$ ) and the momentum in that direction ( $p_x$ ), the mathematical equivalent of the above statement is

$$\Delta x \cdot \Delta p_x \geq \hbar,$$

where  $\Delta x$  and  $\Delta p_x$  represent the uncertainties in the values of  $x$  and  $p_x$  respectively. See Schiff at p. 8, equation (3.1). A mathematical formulation of the Uncertainty Principle, as applied to a wave packet, is given on pp. 60-61 of Schiff.

An alternative mathematical statement of the Uncertainty Principle is:

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<sup>‡</sup> W. HEISENBERG, NOBEL PRIZE FOR PHYSICS, 1932, FOR THE CREATION OF QUANTUM MECHANICS, THE APPLICATION OF WHICH HAS, *INTER ALIA*, LED TO THE DISCOVERY OF ALLOTROPIC FORMS OF HYDROGEN.



“The *extension* of a wave  $\psi(x)$  and that of its Fourier transform  $\varphi(p)$ , where  $\varphi(p) = (2\pi\hbar)^{-1} \int_{-\infty}^{\infty} dx \psi(x) e^{-ipx/\hbar}$ , cannot *simultaneously* be made *arbitrarily small*.”

See p. 130 of Messiah (1958).

It is important to observe that Heisenberg's Uncertainty Principle is no mere philosophical doctrine subject to arbitrary unscientific interpretations. Rather it is established from a proper mathematical basis as is evident from the original formulation in a paper published by Heisenberg (1927). Due to the fact that the applicant appears to have drawn erroneous conclusions based on a misunderstanding of the Uncertainty Principle, in order to provide an objective counterbalance, the examiner deems it appropriate to include copies of both the original paper in German (Heisenberg (1927)) *and* a translation of it (Wheeler and Zurek (1983)) with this response.

The *true* significance of the Uncertainty Principle is most clearly brought out by the following quotations from Tolman (1979) and Merzbacher (1961):

“The most striking consequence of Heisenberg's Uncertainty Principle is the *indeterminacy* which it introduces into the possibilities of physical prediction. In the classical mechanics we grew accustomed to the idea that an exact knowledge of the coordinates and momenta of a system at a given initial time would then make it possible, with the help of the equations of motion, to make an *exact* prediction as to the future behavior of the system. We now see, however, that *such exact knowledge* of the initial values of both the coordinates and momenta *is not possible*, and *hence must give up our older ideas* of the possibility of exact prediction and of a complete causal dependence of the later on the earlier behavior of a mechanical system.

Such a conclusion produces a *drastic change in the ideology of science ...*” Tolman *op. cit.* at p. 187.

And:

“The probability doctrine of quantum mechanics asserts that the *indetermination*, of which we have just given an example [i.e. the double-slit experiment that is described in greater detail below] is *a property inherent in nature and not merely a profession of our temporary ignorance from which we expect to be relieved by a future better and more complete theory.*” (Emphasis added). Merzbacher *op. cit.* at p. 12.

Turning attention to Dürr et al.’s article in *Nature*<sup>★</sup>, entitled “Origin of quantum-mechanical complementarity probed by a ‘which-way’ experiment in an atom interferometer,” it is noted that therein is described an experiment with an atom interferometer which splits an incoming beam of atoms A into separate beams B and C, allows the split beams to propagate, and then again splits each of beams B and C into beams D, F and E, G, respectively. An interference pattern is produced by the overlap of beams E and F in momentum space. Using a which-way detector to monitor the beams destroys the interference pattern. The article emphasizes that no mechanical double-slit is used, no position measurement is performed, and because the atom is not localized the uncertainty principle cannot be “invoked.” See p. 36 of the article. Rather than invoke the Heisenberg Uncertainty Principle, the article suggests an *alternative explanation* for this effect in terms of correlations between the which-way detector and the motion of the atoms. *In no way does this show, nor does the article say, that the Uncertainty Principle is untenable.*

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<sup>★</sup> Dürr, S., Nonn, T. and Rempe, G. (1998) *Nature*, vol. 395, pp. 33-37. See attachment 58.

It is observed that the above experiment is *not* the same as the well-known double-slit experiment where the passage of a single electron or photon through the slits creates a diffraction or interference pattern on a screen placed behind the slits opposite to a source of the electron or photon. Any attempt to detect which of the two slits the electron or photon actually passes through destroys the diffraction pattern and the traditional explanation for this is based on the Heisenberg Uncertainty Principle as shown, e.g., in Feynman et al. (1965) at pp. 1-6 to 1-9. In light of the fact that the applicant has failed to clarify as to why an experiment using an atom interferometer is relevant to explaining the effects produced in a double-slit experiment and, further, in the absence of any support in the article to explicitly negate the Heisenberg Uncertainty Principle, applicant's statements concerning that principle remain no more than untested opinions.

**15. Response to the argument that Feynman's derivation of the Bohr radius is**

**"flawed"**

On pp. 31-32 of the amendment, applicant contends that the Feynman "derivation" of the size of the hydrogen atom is flawed and raises five issues to which a point-by-point response follows. Initially, it is noted that the purpose of Feynman's derivation was to demonstrate a *back-of-the-envelope estimate* that would give some idea of the size of the hydrogen atom in its ground state in light of the Heisenberg Uncertainty Principle. To quote Feynman et al. (1965) at p. 2-5:

"We now consider another application of the uncertainty relation . . . . It [i.e. the application] must not be taken too seriously; *the idea is right* but the analysis is not very accurate. The idea has to do with the determination of the size of atoms, and the fact

*that, classically, the electrons would radiate and spiral in until they settle right on top of the nucleus. But that cannot be right quantum-mechanically because then we would know where each electron was and how fast it was moving [i.e. in violation of the Heisenberg Uncertainty Principle]."*

It is noted that a *fully accurate* derivation of the Bohr radius *is* given in Pauling and Wilson (1985) at pp. 121-124 and 140 as described in the Appendix attached to the previous office action. This *accurate derivation fully confirms the value of the Bohr radius that Feynman obtained* by his method. Applicant's arguments must be read in this context.

With respect, then, to applicant's point "1" (see pp. 31-32 of the amendment), contrary to applicant's position, it is noted that it is routine to use "order of magnitude" estimates when doing a "back-of-the-envelope" calculation. Not only Feynman but also others of skill, such as Schiff (1968) at pp. 10-12, routinely use it with the full understanding that it does not supplant an accurate derivation. It is analogous to doing a pilot project before embarking on a more detailed costly venture. In scientific research, it enables one of skill to assess the relative significance of different variables or parameters and points to those that must be considered in the more detailed program. Likewise, in the art it is observed that it is routine to do a rough estimate before embarking on a more accurate derivation. The justification is whether the final result tallies with the result of a fully accurate derivation - which it *does in this case* as mentioned above. The confidence placed on such an estimate depends on the skill level of the practitioner in the art. With regard to the latter, Feynman's credentials as stated on p. 9 (see, esp., the footnote) of this response stand as an adequate testament. It is quite safely stated that one of such skill would have known the difference between uncertainty in a variable and the variable itself prior to doing an estimate.

With respect to point “2”, contrary to applicant’s position, there is no inconsistency in using the formula  $(1/2)mv^2$  for the kinetic energy in quantum mechanics. To see this, recall that in quantum mechanics (see, e.g., Merzbacher (1961) at p. 4) the kinetic energy ( $T$ ) and linear momentum ( $\mathbf{p}$ ) operators are defined as  $-(\hbar^2/2m)\nabla^2$  and  $(\hbar/i)\nabla$ , respectively. See p. 3 for the meaning of the symbols. The bolded symbols denote vector quantities. It follows that:

$$T \equiv -(\hbar^2/2m)\nabla^2 = (1/2m) (\hbar/i)\nabla \cdot (\hbar/i)\nabla = (\mathbf{p} \cdot \mathbf{p})/2m.$$

That is, the kinetic energy variable corresponding to the operator  $T$  is the same as the variable corresponding to the operator  $(\mathbf{p} \cdot \mathbf{p})/2m$  which, since linear momentum is  $m\mathbf{v}$ , yields  $(m\mathbf{v} \cdot m\mathbf{v})/2m$  or  $(1/2)mv^2$ .

With respect to point “3”, contrary to applicant’s position, no problem is seen in arriving at an exact answer by doing a back-of-the-envelope type of calculation. One should not expect it, but it hardly negates the estimate. If anything, it may strengthen confidence in the estimation procedure. The ultimate justification lies in the fact that the estimate is verified by an accurate derivation, which, as stated previously, *is the case here*.

With respect to point “4”, it is not understood as to what the point of disagreement is given that applicant himself admits that the result of his own derivation agrees with Feynman’s result.

With respect to point “5”, this is based on applicant’s opinion that the Heisenberg Uncertainty Principle is “not based on physics. In fact, it is nonsensical . . . .” See p. 31 of the amendment. Since no experimental evidence has overthrown that principle and applicant’s interpretation of Dürr et al.’s article in *Nature* (attachment 58) regarding the principle was

shown above to be in error (see § 14), applicant's opinion remains untested and, in the absence of objective evidence, cannot be given patentable weight.

#### **16. Applicant description of the prior art is incorrect**

The applicant has, in more than one instance, failed to correctly describe the prior art. As a case in point, attention is drawn to p. 64 of a document authored by applicant and submitted as attachment 9 along with his amendment. Therein, the applicant states that "the many solutions of the hydrogen molecule based on quantum mechanics have many flaws." As an instance of such alleged flaws, applicant points to two papers by Kolos and Wolniewicz (1964) entitled "Accurate adiabatic treatment of the ground state of the hydrogen molecule" (hereafter, Paper I) and Kolos and Wolniewicz (1968) entitled "Improved theoretical ground-state energy of the hydrogen molecule" (hereafter, Paper II), in which "[they] use a [wave function having] 100 terms and an effective nuclear charge of 1.072."

Firstly, it is noted that the references deal with the energy levels of the two electrons in a hydrogen *molecule* ( $H_2$ ). This problem is very different from calculating the energy levels of a single electron in the hydrogen *atom* (H) due to a *crucial* interelectronic interaction that *must* be taken into account in  $H_2$ . Such an interaction is *absent* in H. This shows that the applicant has *failed* to compare his work with the *closest prior art*. It is apparent that applicant has clouded his alleged evidentiary showing with *irrelevant* data.

Secondly, it is observed that applicant's designation of " $\alpha$ " as an "effective nuclear charge" is *not* found in the cited reference papers. Instead, the references disclose an

“exponent,”  $\alpha$ , in the wave function. Rightly or wrongly, applicant has introduced his *own interpretation* of the prior art instead of letting it speak for itself.

Thirdly, a scrutiny of the above cited papers shows that applicant probably refers to Paper II. In that paper, Kolos and Wolniewicz use a 100-term wave function and refer to Paper I for details regarding the exponent. In Paper I, an 80-term wave function where the exponent  $\alpha$  is 1.072 is disclosed. See Table II at p. 3668 therein. The applicant, however, *fails* to explain how the cited data supports his allegation of an alleged flaw in quantum mechanics.

On the other hand, the examiner notes that, with a 100-term wave function having the above-stated characteristics, Kolos and Wolniewicz calculated a binding energy,  $D_0$  (referred to by applicant as a “bond energy”), of  $36117.4 \text{ cm}^{-1}$  which they state is “ $3.8 \text{ cm}^{-1}$  larger than the experimental value.” See p. 409 in Paper II. This means that the error in the calculated bond energy is about *one part in ten thousand or 0.01%* compared to the experimental value. The same conclusion regarding the accuracy of the theoretical value is drawn from the results for the binding energy using an 80-term wave function as disclosed in Paper I. Thus the calculated binding energy from that wave function is  $38297.1 \text{ cm}^{-1}$  compared to the experimental value of  $38292.9 \pm 0.5 \text{ cm}^{-1}$ . See Table VIII on p.3673 in Paper I. This is a remarkable level of accuracy considering the formidable integrals for *interelectronic interactions*, i.e. integrals over  $1/|\mathbf{r}_1 - \mathbf{r}_2|$ , that had to be numerically evaluated in the course of the calculation as well the use of such approximations as the adiabatic approximation, due to

Born<sup>‡</sup>, in which nuclear and electronic motion are separated in order to facilitate calculations. This represents no flaw in quantum mechanics, as alleged by the applicant.

Applicant also states on p. 64 of the same attachment that “[w]hereas, Kolos and Roothaan use an effective nuclear charge of 1.197, and their predicted bond energy is 30% less than the experimental value.” The examiner has consulted that reference, Kolos and Roothaan (1960) entitled “Accurate electronic wave functions for the H<sub>2</sub> molecule.” It appears that there is *no* indication anywhere in the reference regarding applicant’s cited value of the “effective nuclear charge” of 1.197 or his allegation about the 30% discrepancy in the “bond energy” value between theory and experiment. In fact, Kolos and Roothaan computed a binding energy of 4.7466 and 4.7467 eV (i.e. electron volts) compared to the experimental value of  $4.7466 \pm 0.0007$  eV. See p. 225, right-hand column in the reference. The match between theory and experiment for the binding (applicant’s “bond”) energy is *nearly perfect* thus rendering applicant’s statement of a 30% discrepancy in that energy value to be an unsupported allegation.

It is concluded that, contrary to applicant’s imputation, quantum mechanics *cannot* be flawed if, especially, in the very examples cited by applicant himself, there is an accurate match between quantum theory and experiment. *It is apparent that applicant has confused the advances that are made in a sound scientific theory, such as quantum mechanics, as being indicative of so-called flaws. Moreover, under careful scrutiny, these “flaws” appear to be unsupported allegations.*

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<sup>‡</sup> M. BORN, NOBEL PRIZE FOR PHYSICS, 1954, FOR HIS FUNDAMENTAL RESEARCH IN QUANTUM MECHANICS, ESPECIALLY FOR HIS STATISTICAL INTERPRETATION OF THE WAVE FUNCTION.



**17. Response to the argument that the Schrödinger equation “fails” and that Dirac’s quantum electrodynamics is “fatally flawed”**

Applicant has alleged that “the success of quantum mechanics can be attributed to *ad hoc* assumptions.” See page 46 of the amendment. However, it is observed that it is the applicant who has made such assumptions on an unprecedented scale. As evidence, the examiner refers to the discussion in §§ 3 through 10 in this response. In particular, reference is made to applicant’s postulate of energy levels of the electron in a hydrogen atom characterized by having fractional integer principal quantum numbers without *any proper* scientific or mathematical basis as previously discussed.

Prior to responding to applicant’s allegation that the Schrödinger equation “fails to solve the hydrogen atom correctly” and that Dirac’s quantum electrodynamics is “fatally flawed” (see pp. 44 and 52 of the amendment), it is noted that the applicant has clearly *not* grasped the way *proper* scientific theories are formulated. An attempt to explain *all* experimental data down to the “last decimal point” in one fell swoop *via* a grand theory may result in improper mathematics and highly distorted science that cloud the credibility of such a theory. As a case in point, the examiner points to applicant’s improper theory discussed at length throughout this response. On the other hand, generating a theory which, in a first pass, incorporates relatively more important effects, permits one to have a better chance in coming with up with a sound theory that is grounded in correct mathematics and is capable of scientifically progressive refinement to include all other effects until the experimental results are essentially fully accounted for. The basic scientific belief is that such progressive refinement, though seemingly slow and cautious, will nevertheless carry one along until the “final” goal is attained.

The examiner submits that *this is exactly what happens in quantum mechanics* as evidenced by the numerous citations quoted and discussed in this response, and, as further elaborated below.

In quantum mechanics, corrections are systematically incorporated through a progressively refined series of equations. The general idea here is to account for the “biggest chunk” of experimental data *while preserving proper mathematical and scientific basis in so doing*. In keeping with this, note the trend in which the Schrödinger equation [i.e.  $i\hbar \partial\psi/\partial t = \mathcal{H}\psi \equiv -(\hbar^2/2m)\nabla^2\psi + V(\mathbf{r})\psi$ ] gives the following formula for the energy levels of the electron in a hydrogen atom, setting aside corrections due to special relativity and electron spin, i.e.:

$$E_{\text{Schrödinger}} = -(Z^2/n^2)\mathcal{R},$$

where the meaning of the symbols is given on p. 3. The justification for this is that these two corrections account for *only (approximately) one part in one hundred thousand or 0.001%* of the total (binding) energy of the electron in the ground state of the hydrogen atom. Indeed, that is why these corrections are collectively refined to as the “*fine* structure” corrections. See Pauling and Wilson (1985), p. 209. Next comes the Dirac equation which is:

$$i\hbar \partial\psi/\partial t = \mathcal{H}\psi \equiv \{ \boldsymbol{\alpha} \cdot [ c(\hbar/i)\nabla + e\mathbf{A} ] + \beta mc^2 - e\varphi \} \psi,$$

where  $\boldsymbol{\alpha}$  is a vector operator that accounts for electron spin,  $\beta$  is a  $4 \times 4$  matrix,  $\varphi$  and  $\mathbf{A}$  represent the scalar and vector potentials (of an external electromagnetic field), respectively, which satisfy Maxwell’s equations. See, e.g., equation (10.1) at p. 47 in Bethe and Salpeter (1977) or equation (1) at p. 104 in Heitler (1984). The solution to the above equation yields the energy levels of the electron in a hydrogen atom with the incorporation of the fine

structure corrections and is expressed by the following formula (see equation (17.1) at p. 83 in Bethe and Salpeter):

$$E_{\text{Dirac}} = mc^2 \left[ 1 + (\alpha Z / \{n - k + \sqrt{k^2 - \alpha^2 Z^2}\})^2 \right]^{(-1/2)},$$

where  $\alpha$  is the fine structure constant and  $k$  is a quantum number whose values are 1, 2, 3, ...,  $n$  (p. 84 in Bethe and Salpeter). The meaning of the other symbols is given on p. 3. It will be noted that the term  $\alpha Z$  is about 1/137 for the hydrogen atom (where  $Z$  is 1), which is relatively small compared to unity. In light of this, Bethe and Salpeter have expanded<sup>¶</sup> the above formula for the energy,  $E_{\text{Dirac}}$ , in powers of  $\alpha^2 Z^2$ , *not*  $\alpha Z$ , to obtain an expression for the energy that shows the leading term in that expression to correspond to  $E_{\text{Schrödinger}}$  followed by terms accounting for the fine structure corrections. Thus:

$$E_{\text{Dirac}} \approx - (Z^2/n^2) \Re \left[ 1 + \{(\alpha Z)^2/n\} \{ (1/k) - (3/4n) \} + \{(\alpha Z)^4/3n\} \{ (15/8n^3) + (3/4k^3) + (3/2nk^2) + (3k^2/4n) - (9/2n^2k) \} + O\{(\alpha Z)^6\} \right].$$

See equation (17.2) at p. 84 in Bethe and Salpeter. The first term above,  $- (Z^2/n^2) \Re$ , is readily seen to be equal to the energy given by the Schrödinger equation,  $E_{\text{Schrödinger}}$ . The second term is seen to be  $(\alpha Z)^2/4$  of  $E_{\text{Schrödinger}}$ . The value of 1/4 for the coefficient of  $(\alpha Z)^2$  is obtained by noting that  $n = k = 1$  and  $Z = 1$  for the ground state of the hydrogen atom. See p. 84 in Bethe and Salpeter. Substituting the value of  $\alpha$  (from p. 3) into  $(\alpha Z)^2/4$ , it is seen that the correction is 0.0000133 of the energy given by the Schrödinger equation. The third term is seen to be  $(\alpha Z)^4/8$  of  $E_{\text{Schrödinger}}$ . Substituting the value of  $\alpha$ , as before, into  $(\alpha Z)^4/8$ ,

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<sup>¶</sup> For example, by a Maclaurin series expansion according to which an analytic function  $f(x)$  can be expressed as  $f(x) = \sum_{n=0}^{\infty} (x^n/n!) f^{(n)}(0)$ , where  $f^{(n)}(0) \equiv d^n f(x)/dx^n |_{x=0}$ .

it is seen that the correction is 0.000000000355 of the energy given by the Schrödinger equation.

These corrections are thus seen to be so relatively tiny that it is well worth the price for obtaining a *mathematically and scientifically proper equation such as the Schrödinger equation*. It is *not* that one cannot incorporate the relatively *minor* corrections due to special relativity and electron spin into the Schrödinger equation insofar as the energy of an electron in a hydrogen atom goes. Rather, it is far better to focus on the proper science and mathematics involved in the creation of the Schrödinger equation and leave incorporation of the minor corrections in the next phase of quantum mechanics, *viz.* the Dirac<sup>‡</sup> equation which *does account for special relativity and electron spin*.

Lastly, we have quantum electrodynamics whose equations account for all that the Dirac equation does and, additionally, incorporates a “radiative correction” which also has a relatively tiny value. The purpose of this correction is to account for the experimental effect known as the “Lamb shift.” Although, initially, quantum electrodynamics had to contend with divergences in the self-energy calculations for the electron, in modern quantum electrodynamics, such divergences are removed by the technique of “covariant renormalization” as described at p. 92 of Bethe and Salpeter and by Heitler (1984). As evidence of the remarkable accuracy of the equations of quantum electrodynamics, attention is drawn to Table 3 at p. 107 of Bethe and Salpeter, which discloses that, for the hydrogen atom, quantum electrodynamics yields a theoretical value of  $1057.13 \pm 0.13$  Mc/s compared to the experimental value of  $1057.77 \pm 0.10$  Mc/s for the Lamb shift. This means that the

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<sup>‡</sup> P.A.M. DIRAC, NOBEL PRIZE IN PHYSICS, 1933, FOR THE DISCOVERY OF NEW PRODUCTIVE FORMS OF ATOMIC THEORY.

discrepancy between the theoretical value and the experimental value is *six parts in ten thousand or 0.06%* which, given the great care exercised in executing proper mathematical and scientific procedure, is more than sufficient to render applicant's arguments against quantum mechanics to be clearly less than persuasive. It is only fair that applicant's remarks regarding the alleged "failure" of the Schrödinger equation and the alleged "fatal flaw" in the Dirac equation be evaluated against the background of the above discussion.

In response then to applicant's allegations of the specific deficiencies of the Schrödinger equation and the Dirac equation the following remarks apply in *addition* to the comments *already made* in preceding §§ 2-5, 7, 9 and 13.

The natural way in which integer values of the principal quantum number arise in the solutions for the Schrödinger equation has already been discussed above in § 9. With respect to the postulational nature of the Schrödinger equation, none state it better than Pauling and Wilson (1985) at p. 52:

*"No arbitrary postulates concerning quantum numbers are required in this calculation [of the values of the energies of the stationary states of a system]; instead integers enter automatically in the process of finding satisfactory solutions of the wave equation.*

*For our purposes, the Schrödinger equation, the auxiliary restrictions [i.e. the boundary conditions and the normalization condition] upon the wave function  $\psi$ , and the interpretation of the wave function are conveniently taken as fundamental postulates, with no derivation from other principles necessary."*

Regarding applicant's assertion of the lack of Lorentz invariance of the Schrödinger equation, the examiner notes that Lorentz invariance is nothing other than requiring conformance with the provisions of special relativity, an issue which has been discussed and

addressed in the preceding paragraphs in this section. It appears that applicant's allegations reflect a lack of understanding of the recognized fact that the Schrödinger equation *correctly* accounts for the bulk of the ground state energy of the electron, i.e. 99.999% of the energy, as explained above. The remainder is obtained *systematically* from Dirac's equation, which is fully in conformance with special relativity (i.e. it is Lorentz invariant), and from the equations of covariantly renormalized quantum electrodynamics as described previously in this section. Upon weighing the full impact of the Schrödinger equation that yields 99.999% of the ground state energy of the electron in the hydrogen atom using proper scientific and mathematical procedures (within a non-relativistic, spin-free framework) *against* applicant's improperly formulated theory of the "hydrino atom" which yields unheard-of and unverified energy states, it is quite apparent that applicant's allegations regarding "failures" and "flaws" in the Schrödinger and Dirac equations, respectively, cannot be given patentable weight. Additional evidence to support the examiner's position regarding the extraordinary value of the Schrödinger equation is seen in the following quotation from p. 198 in Merzbacher (1961):

"Most obviously, we must correct the error made in assuming that the nucleus is infinitely massive and therefore fixed. ... Further, and often more important, corrections are due to the presence of electron spin and the high speed of the electron, which necessitate a relativistic calculation; hyperfine structure effects arise from the magnetic properties of the nucleus; and, finally, there are small but measurable effects owing to the interaction between the electron and the electromagnetic field (Lamb shift). ... *But all are overshadowed in magnitude by the basic gross structure of the spectrum* as obtained in this chapter by the application of

nonrelativistic quantum mechanics to the Coulomb potential [i.e. the application of the Schrödinger equation to the hydrogen atom].” Emphasis added.

The applicant’s reference to the “kinetic energy of rotation” is a distortion of basic scientific terminology. It is noted that, according to Schiff (1968) at pp. 81-82, the formula  $l(l+1) \hbar^2 / 8\pi^2 m r^2$  represents a centrifugal potential energy term that is supplied by the potential energy and is the force required to constrain the electron to move in a path around the nucleus. It is thus not a kinetic energy term at all. Additionally, for an electron in an atom the concept of rotational energy lacks meaning. The electron may undergo translational motion but it cannot rotate or vibrate. And, of course, given the fact that the wave function of the electron in the ground state (or “normal” state as referred to by Pauling and Wilson (1985) at p. 139) of the hydrogen atom is spherically symmetric, it is hardly surprising that the angular momentum quantum number,  $l$ , is naturally zero for the ground state of the electron.

The applicant’s response that the Schrödinger equation predicts the angular momentum of “the ionized electron” to be infinite is erroneous. The ionized electron represents a situation when the electron is no longer “bound” to the nucleus of an atom. Therefore, it is a free particle conventionally represented by the eigenfunction  $e^{2\pi i p x / \hbar}$  with a linear momentum,  $p \equiv mv$ . See Schiff (1968) at p. 101. The angular momentum,  $L$ , is  $I\omega$ , where  $I$  and  $\omega$  are the moment of inertia and the angular velocity, respectively. Thus, absent evidence that  $I$  and/or  $\omega$  are infinite, the angular momentum of a free electron cannot be infinite.

The applicant’s argument that the Schrödinger equation predicts that the excited state rotational energy levels are nondegenerate does not make any sense in the context of the hydrogen atom which does not have any rotational energy levels.

The applicant's position that the Schrödinger equation predicts that the wave function of a highly excited state electron is infinite is without foundation. In fact, a standard expression for the normalization of a free particle eigenfunction or wave function,  $\psi_{\mathbf{k}}(\mathbf{r}) \sim e^{i\mathbf{k} \cdot \mathbf{r}}$ , is known as the delta function normalization (note that the bolded symbols designate vectors). See, e.g., Schiff (1968) at pp. 54-57. It is observed that the preceding expression for the free particle eigenfunction or wave function is identical to the one stated previously ( $e^{2\pi i p x / h}$ ) on p. 43 when  $\mathbf{r}$  is reduced to the one-dimensional space coordinate,  $x$ , given that  $\mathbf{k} \equiv \mathbf{p}/\hbar$  (Schiff (1968) at p. 54). This argument is equally applicable against the applicant's assertion that the ionized electron, which is free particle, cannot be normalized.

Applicant's comments on the Heisenberg Uncertainty Principle and the Correspondence Principle do not indicate as to what their relevance, if any, is to the applicability of Schrödinger's equation to the hydrogen atom.

The comment that the "Schrödinger equation is not consistent with conservation of energy in an inverse potential field" is clearly off the mark since the Virial Theorem, *via* kinetic energy is minus one-half of the potential energy, clearly holds for the Schrödinger equation. In the particular case of the hydrogen atom in which an electron interacts with a proton nucleus *via* a Coulomb potential ( $\sim 1/r$ ), such a demonstration of the Virial Theorem is shown by Pauling and Wilson (1985) at pp. 145-146.

Applicant's statement that "the Schrödinger equation permits the electron to exist in the nucleus" is baseless. Such a statement reflects the fact that the applicant has *confused the wave function,  $\psi$ , which is required to be finite at the nucleus for the ground state  $n = 1, l = 0$ , with the probability density,  $4\pi\rho |\psi^* \psi|$ , which is zero at the nucleus located at  $\rho = 0$ ,  $\rho$  being the radial space coordinate. It is the probability density, not the wave function, which properly*



describes the probability of finding the electron. The examiner had specifically pointed this out in Endnote 1 in the previous office action. It is noted that applicant has repeated the same erroneous argument at pp. 48-49 of the amendment.

Contrary to applicant's response, the issue at hand is the computation of the energy levels of the hydrogen atom not the scattering of electrons from hydrogen. These are two separate problems and present discussion of the Schrödinger equation has been conducted with respect to the former problem.

It is unclear as to what applicant means when he says that the probability wave interpretation "gives rise to infinite magnetic and electric energy in the corresponding fields of the electron." Perhaps, the applicant is referring to the infinite "self-energy" of the electron. But this is also true in classical electromagnetic theory where it arises from the point charge nature of a classical charge. The issue is resolved in quantum electrodynamics by a covariant renormalization procedure as described in Bethe and Salpeter (1977) at p. 92 and by Schiff (1968) at p. 527.

Applicant's remarks concerning the "unfounded notions" upon which the Dirac equation is allegedly based must be contrasted with one of the most spectacular achievements of that equation *viz.* the prediction of a new particle, the positron, an antiparticle to the electron. In fact, Anderson<sup>‡</sup> first experimentally detected the positron in 1932 just as had been predicted by the Dirac equation. See Heitler (1984) at p. 111.

It is apparent that the success of quantum mechanics with its sequence of increasingly

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<sup>‡</sup> C. D. ANDERSON, NOBEL PRIZE FOR PHYSICS, 1936, FOR HIS DISCOVERY OF THE POSITRON.

refined and powerful theories whose predictions have been confirmed by many experiments over decades of scientific research in this past century outweighs the criticisms leveled against it by the applicant. In the words of a classic graduate level textbook, Schiff at p. 1, cited earlier in § 2:

“At the present stage of human knowledge, quantum mechanics can be regarded as the fundamental theory of atomic phenomena”. Emphasis added.

With respect to the alleged “fatal flaw” in the Dirac equation (see p. 52 of the amendment), the paper<sup>★</sup> entitled “Recent developments in the theory of the electron,” cited by applicant, points out that the theory of Quantum Electrodynamics, as originally developed by Tomonaga, Schwinger and Feynman (who jointly received the Nobel Prize in Physics in 1965 for this work) upon a framework *created by Dirac’s equation*, is verified by experiments. That is, zero-point oscillation and the magnetic moment of the electron are successfully predicted by that theory. The paper implies that a “classical” theory of the kind envisioned by the applicant could *not* predict positrons and that the energy levels of electrons in an atom can be calculated without any assumptions regarding the “inner” structure of the electron.

**18. Applicant’s linkage of a “crisis” in quantum mechanics with the shattering of the doctrine of the big bang origin of the universe is erroneous**

Applicant has stated on p. 46 of the amendment that “quantum theory is now in a state of crisis” and has simultaneously referred to an observation that has allegedly “shattered” the “doctrine” of the origin of the universe as a big bang. Such a statement lacks probative value

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<sup>★</sup> Weisskopf, V. F. (1949) *Reviews of Modern Physics*, vol. 21, no. 2, pp. 305-315. See attachment 59.

since there is no indication of what the big bang theory has to do with the quantum mechanics of an electron in a hydrogen atom. Even if it somehow did, it is contended that applicant's assertion that the big bang theory is "shattered" is incorrect. Thus, attention is drawn to a recent article in *Nature* by Srianand et al. (2000) entitled: "The cosmic microwave background radiation temperature at a redshift of 2.34." The article establishes that experimental observations of the ultra-violet and visible spectra of a quasar are in *accord* with value of the temperature of residual radiation as predicted by the big bang theory. Therefore, applicant's linkage of quantum mechanics with a factually erroneous statement regarding the big bang theory has no bearing on the validity of the former.

**19. The Turner and Dhandapani Declarations with respect to NMR experimental data lack probative value**

Applicant has submitted declarations from Turner and Dhandapani attesting to experimental support for "hydrino hydride" compounds containing the alleged "hydrino atom." See attachment 50.

According to paragraph 8 of the Turner declaration, dated May 18, 2000, when applicant's compounds were analyzed using MAS  $^1\text{H}$  NMR (i.e. "Magic Angle Spinning Proton Nuclear Magnetic Resonance") spectroscopy, signals were observed in the region of -4 to -5 ppm upfield with respect to a TMS signal. It is noted that TMS, or tetramethylsilane, is usually used as a standard in this spectroscopy for calibrating signals. The TMS signal identifies the "zero" (i.e. 0 ppm) of the scale. The declarant asserts that: "Since 1978 I have been primarily conducting NMR scans and I have never observed signals in the region of -4 to -5 ppm before."

According to Table 1 in paragraph 14 of the Dhandapani declaration, dated August 14, 2000, a MAS  $^1\text{H}$  NMR spectroscopic analysis of applicant's hydrino hydride compounds showed "novel features" at signals of -2.5, -3.2, -4.1 and -4.4 ppm. Further, in paragraph 16 Dhandapani asserts that these upfield shifts (from the TMS signal) allegedly indicate that "new lower energy states for the hydride ion (hydrino hydride ion) may exist."

These declarations are deemed to *lack probative value* for the following reasons:

- Contrary to the assertions of the declarants, upfield shifts in the region from -4 to -5 ppm are known in the prior art. Thus, attention is drawn to Hayashi et al. (1997, Papers A - C) which disclose a signal of  $-4.1 \pm 1.0$  ppm for the hydride compound  $\beta\text{-Mg}_2\text{NiH}_4$  in a MAS  $^1\text{H}$  NMR spectroscopic experiment using TMS as a standard. See the abstract and p. 67 in Paper "A". This paper provides a factual contradiction of, and shows the error behind the implication of, Turner's assertion that signals of -4 to -5 ppm have "never" been observed other than in applicant's claimed compounds.
- There is a complete break in logic in the implication of the Turner and Dhandapani declarations that since upfield shifts have "never" been observed in the region between -4 to -5 ppm by Turner, therefore, these signals in this region must support the existence of applicant's "hydrino hydride" compounds. The declarants have *not* objectively established that a host of other species, including contaminants, could not have contributed to such signals rather than the alleged "hydrino hydride" compounds.
- Given the importance of these declarations, it is significant that declarant Dhandapani concludes the declaration with a *tentative* suggestion that a "hydrino

hydride ion" *may* exist. Surely, if the declarant truly believed that the observed upfield shifts are "novel features," why the hesitation in arriving at a positive conclusion?

Based on the above discussion, it is evident that the Turner and Dhandapani declarations lack probative value. Furthermore, since the NMR data cited in attachments 7 and 18-22 are underpinned by the veracity of the above declarations, it is evident that these data must be held to be unpersuasive.

**20. Calorimetric experiments purporting to demonstrate "apparent excess heat" cannot be accepted *uncritically***

It is the examiner's position that one of ordinary skill would recognize that apparent excess heat during electrolysis is a phenomenon that cannot be accepted *uncritically*. As evidence, attention is drawn to the following observations in a series of publications by Hansen et al. (1995, 1997, 1998, Papers I - III):

"Excess heat could come from unsuspected chemical reactions, mechanical or electrical work, experimental error, nuclear fusion or new chemistry." This paper will present the results of experiments showing that reactions of hydrogen and oxygen at the electrodes probably account for *many* previous observations of excess heat....

*Mills et al. make the only clear claim to such an excess heat rate, but their use of pulsed power and uncertainty about calorimetric accuracy complicates interpretation of their work. ... None of the publications by Mills et al. ... contain details on the design and construction of their calibration heaters. Electrical calibration of heaters is notoriously difficult, particularly at high power.*" Emphases added. (See pp. 6973-6974 in Paper I.)

“Claims of ‘excess heat’ from measurements of the heat of electrolysis at several watts of power are largely based on use of poorly characterized, isoperibol, heat-conduction calorimeters with single-point temperature sensors. . . . Observations of ‘excess heat’ are *more likely* due to calorimetric errors *than* to violations of the laws of thermodynamics, or to *known or unknown nuclear or chemical reactions.*” (Abstract and pp. 14-15 in Paper II.)

“Our work shows that, in the absence of extremely vigorous mixing, temperature gradients inside the calorimetric vessel, coupled with failure to measure properly the average wall temperature of the calorimetric vessel, can cause *surprisingly large errors in heat rates* calculated with the heat conduction principle. . . . Thus, our conclusion that ‘faradaic efficiency must be accurately determined and calorimetric accuracy must be demonstrated while the cell is producing excess heat’ still stands if *compelling* evidence is to be obtained.” (See p. 226 in Paper III.)

## **21. Applicant’s calorimetric experimental data are not persuasive**

In support of inventions involving the “hydrino atom,” the applicant has submitted experimental data which are alleged to be explainable by a postulated “hydrino atom.” The applicant further claims that experimental calorimetric data obtained by independent agencies such as NASA, Westinghouse STC and Brookhaven National Laboratory support his invention.

Turning to the data disclosed in the *internal* NASA Technical Memorandum 107167 submitted in attachment 29 of his amendment, it is significant, that even in this case when a party apparently receptive to the applicant’s ideas conducted experiments to measure

apparent excess heat in an electrolytic cell containing applicant's theorized "hydrino atoms," the outcome was far from being compelling.

In particular, the NASA memorandum is unpersuasive because the attempted replication of the phenomenon of apparent excess heat in a light water-Ni-K<sub>2</sub>CO<sub>3</sub> electrolytic cell simply does not stack up to the test results from "a private corporation" which, in reality, appears to be applicant's own company. Thus the excess heat of 11 W *maximum* for the replicated experiment should be contrasted with the substantially larger excess heat of 50 W for a continuous period exceeding hundreds of days for the tests run by the applicant's company (Abstract of the memorandum). It is noteworthy that to the authors of the Memorandum the outcome was such that they concluded that "although our data admits of the existence of an unusual source of heat it falls far short of being compelling." See page 7 of the Memorandum. This is hardly credible evidence of *reproducibility* of scientific data. The authors go on to say (on p. 7):

"To delimit the alternatives, we have examined the following factors considered in the literature as potential causes of multiwatt level, steady state, apparent excess heat in the present type cell:

1. Unrecognized nonlinearity in the cell thermal conductivity at low temperature differential, leading to erroneous extrapolation for the excess heat.
2. Injection of heat into the cell by thermoelectric pumping.
3. Exothermic chemical reactions involving the nickel cathode.
4. Heat from hydrogen-oxygen recombination within the cell."

After further discussion, the authors conclude that: "Following the principle of simplest explanation that fits the data on hand, recombination becomes the explanation of choice."

Such a conclusion based on an "ordinary" explanation stands in direct contrast to applicant's belief that the data is explained by the supposed release of heat by the formation of "hydrino atoms."

Another piece of evidence cited to explain "excess heat" by the "hydrino atom" is the experiment conducted and reported out by Peterson of Westinghouse STC, submitted as attachment 38. Note that the "Hydrocatalysis Power Corporation" or HPC referred to in the Westinghouse STC report is none other than an earlier name for the applicant's present "BlackLight Power, Inc." company. Although excess heat from electrolytic cells furnished by HPC was reported, the *credibility* of such a result is *undermined* by Westinghouse STC's identification of "*design problems* with the HPC cells [that] make power balance comparisons *uncertain*." See p. 3 of the report. To make matters worse, the Westinghouse STC report concluded that the "[d]ata provided by HPC were *unconvincing* when reviewed by STC experts."

More evidence submitted by applicant to support his claim of "excess heat" in a calorimetric experiment is in a letter from the Brookhaven National Laboratory in attachment 44. In this letter, commenting on an experiment by Noninski, the writer states that "I [did *not* check the calibration of his [i.e. Noninski's] equipment *nor* did I observe his experimental technique in great *detail*." Further on in the letter, the writer concludes that: "The presence of 'excess power' has *not*, however been demonstrated much less 'proved'. A *number* of basic *experiments need to be performed* to eliminate some of the possibilities [i.e. mechanisms other than those based on the postulated 'hydrino atom'] outlined. Finally, there *probably are* many other possible explanations for explaining this thermal anomaly and these would, of course, also *need to be investigated*." Emphases added. It is apparent from the above



that the experimental data cited by applicant is simply not probative of patentability for his claimed invention based on the “hydrino atom.”

**22. Accurate calorimetric experiment disproves applicant’s thesis that apparent excess heat requires an explanation in terms of the postulated “hydrino atom”**

A significant experiment, not reported by applicant, conducted to investigate the possibility of apparent excess heat in an electrolytic cell of the same kind used in the NASA Technical Memorandum, cited in § 21 above, is the one described by Shkedi et al. entitled “Calorimetry, excess heat, and Faraday efficiency in Ni-H<sub>2</sub>O electrolytic cells.”<sup>★</sup> According to p. 1730 of the article:

“The application of *highly accurate and rigorous calorimetry* as presented in this research combined with *proper accounting for the actual Faraday efficiency* clearly indicate that the apparent excess heat observed in these experiments *is a result of neglected conventional chemistry. This conclusion is supported by the lack of any excess heat in the closed cells as well.*”

Emphasis added.

Moreover, according to the abstract on p. 1720 of the article:

“The measured Faraday efficiency is significantly <100%, and *conventional chemistry can account for the entire amount of observed apparent excess heat to within an accuracy of better than 0.5%.*” Emphasis added.

Most tellingly, the article states at pp. 1729-1730 that:

“Even though this research was not intended to test the validity of the Mills and Kneizys theory [cf. § 11 of this response], the results obtained lead to a *clear conclusion*

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<sup>★</sup> Shkedi, Z., McDonald, R. C., Breen, J. J., Maguire, S. J. and Veranth, J. (1995) *Fusion Technology*, vol. 28, pp. 1720-1731.

as to whether or not the *postulated 'hydrino' atoms* or molecules were formed. ... The lack of any volume increase ... *preclude the formation of any such novel atoms or molecules in these cells.*" Emphasis added.

This provides further evidence that not only is applicant's experimental observation of a relatively large amount of apparent excess heat unsupported by others of skill in the present technology, but that, in any event the applicant's flawed theory is also not required to interpret the results of such experiments.

### **23. The Phillips Declaration with respect to "hydrino" formation in a calorimeter lacks probative value**

Applicant has submitted a declaration from Phillips, a co-inventor with the applicant in World patent WO 96/42085 (cited on sheet 3 of 15 in the Information Disclosure Statement filed May 21, 1999 in Paper No. 8 of Serial No. 09/009,294), attesting to experimental support for "hydrino" formation in a calorimeter. See attachment 51. According to paragraph 7 of the declaration, an experiment was performed in the gas phase in a Calvet cell type of calorimeter to test applicant's hypothesis that potassium ions would catalyze the conversion of hydrogen atoms to "hydrino atoms" in a manner consistent with applicant's theory. And, according to paragraphs 10 and 21, the hypothesis is, apparently, "consistent with the present results." These results are average power outputs of 0.5868, 0.5959 and 0.6047 Watts, respectively, as shown in Figures 3-5 of the declaration.

The declaration is deemed to *lack probative value* for the following reasons:

- There is no indication of the detection limit of "excess heat" for the Calvet cell type of calorimeter used by Phillips. Without this information, it is not possible to

determine if the declarant's results are experimentally meaningful. In paragraph 11, the declarant indicates that the calorimeter is "similar" to the one described in "earlier reports" dated January 1, 1996. Since these reports are supplied in attachment 34, the examiner has consulted that attachment only to find that the detection limit is not mentioned. Upon reviewing the Information Disclosure Statement filed May 21, 1999 (e.g. in Paper No. 8, Serial No. 09/009,294), it is seen that in a report cited by applicant entitled "Earth Tech's campaign to replicate one of the BlackLight Power excess heat results," the detection limit for a vapor phase cell calorimeter of the kind used by Phillips is described to be "about 1 or 2 watts." See p. 3 of the "BLP gas-phase replication effort – run 13" of February 18, 1998. It is apparent that since the detection limit of "about 1 or 2 watts" for the calorimeter *exceeds* the average power output of 0.5868, 0.5959 and 0.6047 Watts measured by Phillips, the conclusion of "excess heat" is *unjustified*.

- The control experiment performed by Phillips, which involved substituting helium (an inert gas) for hydrogen while using a source of potassium ions, *fails* to show that it is the formation of a "hydrino atom" catalyzed by potassium ions that is responsible for the alleged "excess heat." See paragraph 16 of the declaration. A proper control experiment would have used a different "catalyst," such as sodium ions, for instance, instead of potassium ions, in the presence of hydrogen, based on applicant's belief that sodium ions will not catalyze the formation of "hydrino atom." See, e.g., attachment 39 and p. 72, line 35 of the specification of Serial No. 09/009,294. The use of an improper control experiment raises serious doubts as to the probative value of the declaration.

- According to p. 3 of Phillips' report in attachment 34, cited previously, an attempt to detect "excess heat" according to applicant's predictions *failed* in an experiment involving a water bath calorimeter. When, as here, remarkable and unheard-of results are alleged, it is only proper scientific procedure to confirm the results by a variety of experimental techniques. The failure of the water bath calorimeter experiment thus leaves unresolved the question of whether the results alleged by applicant are truly experimentally verified. By failing to confirm the allegation of success reported in the present declaration, attachment 34 raises a concern as to the probative value of the declaration under discussion.

In view of the above discussion, it is concluded that the Phillips declaration fails to support applicant's allegation regarding "hydrino" formation in a calorimeter.

#### **24. Applicant's spectroscopic and plasma experimental data are not persuasive**

Attention is drawn to attachment 38 of applicant's amendment. It contains a review of an ESCA (i.e. Electron Spectroscopy for Chemical Analysis) experiment to characterize a nickel electrode on which "hydrino atoms" were allegedly created. The review, by a Westinghouse STC scientist, concludes that:

"In summary, the ESCA data presented by Mills and Good does *not* provide strong evidence for *fractional* quantum states of hydrogen."

On p. 64 of the amendment, applicant directed the examiner's attention to various figures obtained by XPS or ESCA which are alleged to show evidence of "fractional" energy levels. These figures, numbers 17, 19, 21 and 29, have been studied but fail to support applicant's contention in view of the following discussion.

It is observed that in photoelectron spectroscopy (whether UPS - ultraviolet photoelectron spectroscopy, XPS - X-ray photoelectron spectroscopy or ESCA) electrons are ejected from their orbitals by incident photons. Measurement of the kinetic energies of the ejected electrons and knowledge of the energies of the incident photons yields a spectrum of ionization (or binding) energies of electrons from various orbitals as evident from Einstein's law of photoelectric effect, *viz.*:

$$\text{Ionization energy of electron} \approx \text{Incident photon energy} - \text{Kinetic energy of electron.}$$

In XPS and ESCA, due to the relatively high incident energies of the photons, inner shell or core electrons are ejected. The spectra are affected by the environment of the atom from which the electron is ejected. The shape of the bands (or peaks) in the spectra are significant in that they provide information about the bonding characteristics of the orbital from which an electron was ejected. For molecules, the shape of the bands are influenced by many factors such as spin-orbit coupling, vibrational fine structure, dissociation, Jahn-Teller effect and exchange and multiplet splitting.

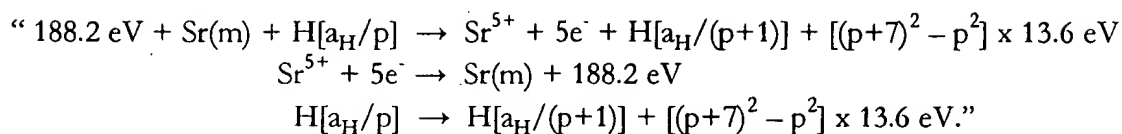
Turning to applicant's spectra as shown in figures 17, 19, 21 and 29, as cited on p. 64 of the amendment, it is immediately evident that some of the assignments are devoid of meaning. Namely, photoelectron spectra conventionally show ionization or binding energies of an electron in a given energy level or orbital and *do not* show transitions between different orbitals or energy levels. Yet such transitions between allegedly fractional quantum number integer energy levels are shown. See, e.g., the transitions indicated as " $\text{H}^-(1/14) - \text{H}^-(1/16)$ " in figure 17 and " $\text{H}^-(1/13) - \text{H}^-(1/16)$ " in figures 19, 21 and 29. Moreover, in figures 21 and 29 such transitions are shown in a portion of the spectrum that is essentially in the noise region and therefore could not have been assigned with confidence. Moreover, in light of the

preceding discussion regarding the factors that affect band shapes and the assignments, it would have been a reasonable expectation on the part of one of ordinary skill in the art that applicant would have provided such details instead of exhibiting a *conclusionary* assignment of transitions to various peaks some of which are hardly visible. In view of the confusion engendered by applicant's spectra, it is deemed that they lack probative value.

It is observed that applicant has alleged that a "plasma [is] formed at 1% of the theoretical or prior known voltage requirement with 4,000-7,000 times less power input power [sic] compared to noncatalyst controls ...," and that "[t]he light output for power input was increased to 8600 times that of the control when argon was added to the hydrogen strontium plasma to form catalyst  $\text{Ar}^+$ ." See p. 4 of a paper authored by applicant Mills in attachment 2. The applicant also alleges that a "plasma is formed for hydrogen-strontium mixtures at an extremely low voltage of about 2 V." See a document co-authored by applicant Mills in attachments 10, 15 and 16 as well as p. 23 of the amendment.

These allegations are deemed to be unpersuasive in light of the following discussion. Noting that a plasma is a gas consisting of charged particles, such as ions and electrons, it is observed that, according to the applicant, the energy required to produce a plasma from a neutral hydrogen atom is allegedly obtained by tapping into its "fractional quantum number energy states" *via* a "non-radiative transition" catalyzed by certain elements. However, as established by the examiner throughout this response, and, especially, in § 9, the applicant has failed to demonstrate that fractional quantum number energy states *even exist*.

The applicant has *postulated* the following reaction mechanism scheme to account for plasma formation from hydrogen using strontium (Sr) as a "catalyst":



Thus see attachment 15, equations 5 to 7 on p. 3 of a paper, co-authored by applicant Mills, entitled "Observation of extreme ultraviolet hydrogen emission from incandescently heated hydrogen gas with strontium that produced an anomalous optically measured power balance."

According to p. 2 of the cited paper, "p" in the above equation is an integer *greater than 1*. Therefore, a *starting reactant* in the first equation above is a "*hydrino atom*,"  $\text{H[a}_\text{H}/\text{p}]$ . This indicates that the applicant *assumes the existence of a "hydrino atom"* in order to show that a plasma is allegedly obtained from hydrogen under rather unlikely conditions. Clearly, this forms no logical argument to establish the existence of a "hydrino atom" from the formation of a plasma.

Moreover, applicant's postulated mechanism requires the formation of  $\text{Sr}^{5+}$  which represents a clearly *unconventional* oxidation state of strontium whose normal oxidation state is  $\text{Sr}^{2+}$  given that it is an alkaline earth metal from Group IIA of the Periodic table.

Furthermore, in view of applicant's own statement that strontium allegedly functions as a "catalyst" (which, by definition, does not itself undergo a chemical change), it is then *contradictory* to show a reaction in which strontium participates as a reactant in that it undergoes conversion from a neutral element (Sr) to an ion ( $\text{Sr}^{5+}$ ).

Additionally, no compelling scientific reason is presented as to why precisely 188.2 eV of energy are required as an input energy in the first equation of applicant's postulated reaction mechanism other than applicant's desire to find a "match." It is noted that applicant's

reaction mechanism is not consistent with achieving a true resonance condition since there appears to be no integer value of “p” consistent with the requirement that the energy for converting Sr “catalyst” to the oxidized form  $\text{Sr}^{5+}$  is resonant with, or is the same as, the energy released in the transition between the “ $\text{H}[\text{a}_\text{H}/\text{p}]$ ” and “ $\text{H}[\text{a}_\text{H}/(\text{p}+1)]$ ” states of the “hydrino atom.”

Applicant has stated that “The mechanism of EUV emission ... is predicted by a solution of the Schrödinger equation with a non-radiative boundary constraint put forward by Mills.” See p. 2 of the cited paper. This statement is clearly incorrect since the applicant uses a classical wave equation, not the Schrödinger equation, as his starting point as discussed by the examiner in § 5 of this response. Since the use of a classical wave equation to describe a quantum phenomena, such as the radiation emitted from an atom, is incorrect (cf. § 5), it is not surprising that applicant reaches an incorrect conclusion regarding “extreme ultraviolet hydrogen emission” from hydrogen gas.

It is concluded that applicant’s reaction mechanism represents a scientifically improper scheme which is indicative of applicant’s expectations of the way things *ought* to be rather than a scheme in which conformity to *logic* and *correct scientific facts* dictate the outcome. In light of the above, any allegation of forming a plasma by using energies given by transitions between non-existent energy states of a “hydrino atom” powered by a 2 V source lacks scientific credibility.

It is observed that the source of the statements cited above from attachments 2, 10, 15 and 16 *emanates from the applicant*. In particular, the allegation of plasma formation at input voltage thousands of times lower than conventionally used voltage is found in papers authored by applicant and his coworkers *none of which appear to have been evaluated for scientific*



*accuracy.* See, e.g., p. 28 of attachment 2 where the papers are said to have been either “submitted” or “in press.” Moreover, experiments appear to have been conducted at the “Institut für Niedertemperatur Plasmaphysik e.V.” by Conrads with funding apparently provided by applicant’s company, BlackLight Power Inc.. (See Dow Jones Newswires October 6, 1999. ★)

In view of the above discussion, it is concluded that applicant’s plasma experimental information lacks probative value.

**25. The Mills Declaration with respect to “extreme ultraviolet spectroscopic data”  
lacks probative value**

Applicant Mills has submitted a declaration, dated June 8, 2001, which claims that he has allegedly obtained “new emission lines” for hydrogen (paragraph 6 of the declaration), has formed a plasma in a cell with “no high voltage source” (paragraph 10), has “confirmed the resonant nonradiative energy transfer of 3·27.2 eV [sic] from atomic hydrogen to atomic potassium,” (paragraph 11) and “conclusively show[n] the spectroscopic observation of the predicted  $H^{-}(1/4)$  hydride ion” (paragraph 12). In the declaration, the applicant cites publications, test reports and meetings. Attached to the declaration are two documents co-authored by applicant Mills entitled “Spectral emission of fractional quantum energy levels of atomic hydrogen from a helium-hydrogen plasma and the implications for dark matter” (Paper I) and “Spectroscopic identification of a novel catalytic reaction of potassium and atomic hydrogen and the hydride ion product” (Paper II), respectively. These documents

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★ Baard, E. (1999) *Dow Jones Newswires*, October 6 reprint.

relate to spectral emission from atomic hydrogen and spectroscopic identification of a "novel catalytic reaction."

The declaration is deemed to *lack probative value* both in view of the discussion in the preceding section (§ 24) regarding applicant's spectroscopic and plasma data and further in view of the following observations. Firstly, it is noted that in Paper I above, the applicant's postulated reaction mechanism for the formation of a hydrogen plasma using helium as a catalyst (see equations 5 to 7 on p. 8 of the paper) is similar to the corresponding mechanism using strontium as a catalyst, barring the use of different energy numbers (eVs) to force a match between the input and output of energy. Since the illogical and scientifically improper nature of applicant's strontium-based reaction mechanism was previously discussed in § 24, the examiner's position as stated there applies equally well against applicant's postulated mechanism in Paper I, especially, with regard to the unconventional oxidation state of  $\text{He}^{2+}$  for helium which is an *inert* element and the applicant's use of an energy input of 54.417 eV in conformance with applicant's expectation of what the reaction mechanism ought to be rather than base it on scientifically justified requirements.

Secondly, it is noted that Paper II above, which deals with extreme ultraviolet emission from incandescently heated hydrogen, is *cumulative* to applicant's information given in attachments 10 and 15 which were previously discussed in § 24.

Thirdly, the interpretation of the "experimental" data obtained by the applicant *hinges crucially* on the existence of *allegedly fractional* integer quantum number energy levels of the electron in the hydrogen atom. Conversely, *it is the applicant's theory* of the "hydrino atom" with fractional integer quantum number energy levels *which predicts* new spectral lines and transitions for atomic hydrogen, the formation of a hitherto unknown species of hydride ion

and the generation of plasma under rather unlikely reaction conditions. Thus, see Papers I and II, cited above. The examiner maintains that since such energy levels have *not* been shown to have any *proper* scientific or mathematical basis, as objectively established by the examiner in § 9 in this response, any prediction from such an improper theory which leads to an assignment of spectral lines is *clearly devoid* of meaning. In light of this, the figures showing spectra and the tables listing transitions between fractional integer quantum number energy levels lack scientific merit.

Furthermore, it would have been a reasonable expectation on the part of one of ordinary skill in the art that the applicant's assignment of "catalytic transitions," such as  $n = 1/2 \rightarrow 1/3$ , etc., which are stated by the applicant himself to be "*autocatalytic*" and have a "higher reaction rate than that of the inorganic ion catalyst" (see p. 8 of Paper I) would logically (due to autocatalysis and the lack of a lower bound on  $1/p$  where  $1/p$  is the fractional principal quantum number,  $n$ , and  $p = 2, 3, 4$ , etc.) lead to a cascade of transitions culminating in the collapse of the alleged "hydrino atom." This issue of the stability of the "hydrino atom" and applicant's unpersuasive arguments given in connection with that is discussed in § 7 of this response. This provides yet another reason as to why the applicant's assignment of transitions is without meaning given that the "hydrino atom," if it ever existed, is unstable.

Applicant's alleged observation of "new" emission lines in the spectrum of the hydrogen atom must be weighed against the following facts given in the previously cited state-of-the-art treatise by Bethe and Salpeter (1977), at p. 12, *viz.*:

"The wave numbers of *many lines* of the spectrum of the hydrogen atom can be measured to an *accuracy of better than one part in a million*. ... After applying corrections for fine structure, etc., [cf. the Dirac equation and the equations of quantum

electrodynamics discussed by the examiner in § 17] the agreement for a *large number of spectral lines is excellent*, to better than *one part in a million* for the  $H_{\beta}$ ."

As evidence, Figures 2 and 3 from pp. 10-11 of Bethe and Salpeter are attached to show that the conventional experimentally obtained lines in the spectrum of the hydrogen atom have been *properly* identified by standard quantum mechanics. It is, therefore, very clear that it is an art-recognized fact that the spectrum of the hydrogen atom has been mapped out thoroughly.

It is noted that the applicant states that his predictions match the "experimental" data to a "remarkable three significant figures" (see paragraph 7 of the declaration). While it is not understood as to what accounts for its being so remarkable, what *is* of significance here is that this degree of "matching" clearly pales in comparison to the predictions of conventional or standard quantum mechanics, as evinced by Bethe and Salpeter's statement cited above, that quantum mechanics has an accuracy of "better than one part in a million." Hence, *standard quantum mechanics is a thousand times more accurate in its predictive power, for the spectral lines of the hydrogen atom, compared to applicant's improper theory*. In light of the above discussion, applicant's allegation of novel experimental data, based on his *improper* theory that *predicts hitherto unseen* spectral lines, is clearly without merit.

Of particular concern is the fact that the declaration and its attachments contain no independent evidence to confirm applicant's allegations concerning the "hydrino atom" or, synonymously, "lower energy hydrogen". That is, the hallmark of scientifically valid information, *viz.*, information that is *reproducible and verified* has *not* been met in this case. This is all the more significant given that, in the present instance, *applicant's claims violate conventionally well-established ideas of modern science* as established throughout this response. While

p. 28 of Paper I cited above refers to the contribution of the Institut für Niedertemperatur Plasmaphysik e.V." in obtaining experimental data on transitions in the hydrino atom, the source providing such data cannot be considered as being independent for the reason stated in § 24 at p. 61.

It is further observed that there is no indication as to whether Papers I and II co-authored by applicant Mills have been independently checked for accuracy as opposed to representing applicant's *own* statements of support for his postulated "hydrino atom" and its alleged experimental manifestations.

The publications, test reports and meetings noted in an attachment to the declaration do not appear to be new and were previously cited by the applicant in the attachments to his amendment in response to the previous office action. These attachments have been discussed at length in this response by the examiner. In the previous sections of this response, the merits of the test reports, alleged by applicant to be "independent," were evaluated and found to be unpersuasive. Thus see §§ 21 and 23.

Contrary to the apparent impression produced by applicant's citing of as many as 23 meetings, a study of the list of meetings shows that, in reality, the applicant made several different presentations in the same meeting or fewer meetings rather than exhibit his ideas in many different meetings. As an example, note that the 4 meetings (meeting numbers 11 to 14) all involved different presentations in the *same* meeting, *viz.*, the 219<sup>th</sup> American Chemical Society meeting. Moreover, the subject matter for meetings 12 to 14 overlap in that they all involve applicant's hydride compounds. Thus his list of 23 meetings drops to 10 distinct meetings. It is further observed that at least some of the presentations in the meetings held

by the American Chemical Society appear to be "poster presentations." In such presentations, there is no requirement that an audience be present.

In view of the above discussion, it is evident that the Mills declaration has no probative value.

## **26. Conclusion**

As established in this response by the examiner, the applicant's scientifically and mathematically improper theory and the lack of probative experimental data do *not* overcome the weight of evidence against the existence of the postulated "hydrino atom." It is, therefore, deemed to be sound and proper to hold applicant's invention, which is based on the postulated "hydrino atom" and its alleged experimental manifestations, to be unpatentable.

**"State-of-the-art" books and journals cited by the examiner**

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